THE

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THE ORGAN OF THE AËRONAUTICAL SOCIETY OF GREAT BRITAIN.

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**Edited for the Council by W. BARNARD FARADAY, LL.B., F.S.S., Barrister-at-Law.**

*All communications should be addressed to the Editor.*

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## Notices of the Aëronautical Society of Great Britain.

### The Society's Lectures.

The Autumn Syllabus of Lectures will begin on Wednesday, October 31st, 1917. Details will be announced later.

### New Members.

The following have been elected to membership of the Society in the various grades as shown :—

*Hon. Fellow.*

W. H. Dines, F.R.S.

*Fellows.*

Dugald Clerk, D.Sc., F.R.S., F. W. Lanchester, M.Inst.C.E., Colonel M. O'Gorman, C.B. (3).

*Hon. Associate Fellow.*

Colonel F. H. Sykes.

*Associate Fellows.*

Lieutenant-Colonel R. K. Bagnall-Wild, A. W. Judge, H. P. Martin, T. G. John, R. Borlase Matthews, R. Richardson, Captain T. E. Robertson, F. R. Smith, J. W. Carr, H. Shaw, Captain de Havilland, W. Brierley, Ernest Robinson, Colonel J. D. Cormack, T. O. M. Sopwith, W. G. Carter, F. Sigrist, Captain W. S. Farren, L. W. Bryant, W. H. Barling, Dr. J. E. Ramsbottom, Thos. Jones. C. W. Alexander, J. M. Heesem, Commander W. Briggs, E. Spencer, W. E. Dommett, C. H. Brooks, S. H. Smith, J. D. Scarpe, H. T. Tizard, J. D. Blondeau, K. Secretan, E. G. Cole, H. C. Fuller, G. H. Hales, C. H. Powell, J. R. Pannell, H. Knowles, Capt. F. S. Barnwell, R. Chadwick, Norman Barrett, H. E. Wimperis, G. P. Bullman, G. E. Barnhart, F. T. Hill, O. P. Monckton, Captain R. H. Verney, E. G. Walker, A. S. A. Ormsby, A. A. Remington, E. Sparshott, O. W. Thomas (53).

*Students.*

Tomatoshi Taku, F. L. Richards, F. G. Reddie, S. Richards, L. Richards, A. H. Tillman, R. W. Sloley, T. E. Davis, R. C. Cross, L. G. Friss, R. P.

Hearder, L. D. Whistler, E. G. Moody, G. K. Carlson, T. P. McNeale, L. J. Pond, L. A. Martin, A. J. T. Ireland, H. Yendall, W. H. Lyne, R. K. Heysham, J. N. F. Morris, R. W. Sutton, W. E. James, J. Shepherd, J. Williams, V. W. Derrington, L. S. Flatman (28).

*Members.*

Margaret Grice, R. W. Corkling, R. O. Cary, V. K. Vyvyan, Captain J. H. Brett, H. G. Selfridge, G. R. Inshaw, A. W. Towse, G. B. Dunn, H. A. Dudgeon, A. Lyall, A. J. Westlake, W. Hooper, A. M. McCall, E. Bartlett, E. H. Knight, C. H. Watkins, L. W. Snell, A. R. Donnison, J. W. Miller, F. W. G. Snook, H. T. R. Gee, E. H. Archer, H. Rutherford, W. W. Hackett, J. S. Grieg, G. N. C. Mann, F. W. Derwick, H. Fulton, C. G. Vokes, H. Waring, A. J. A. W. Barr, Sir W. A. Laurence, Countess of Portarlington, Countess of Drogheda, J. Robertson, T. D. Carpenter, T. Sloper, H. G. Spencer, D. Stevenson, Major F. G. Ogilvie, Sir Vincent Caillard, Lieutenant J. E. Courage, Mrs. H. Johnson, A. J. Wood, R. S. Morrell, A. P. Donnison, W. E. Reid, A. H. Fowler, H. W. Miller, Viola Meeking, Finola Meeking, S. St. Barbe, M. Fick, A. W. Peck, H. Burroughes, G. A. Peck, C. H. Greswell, F. E. St. Barbe, A. J. Rowledge, S. E. Blakeney, T. H. Carter, C. C. Walker, H. Kerr, A. Webb, W. Birchenough, H. Baines, J. Baines, G. W. Lynn, Commander H. T. A. Bosanquet, C. H. Willand, T. W. Willis, Brigadier-General D. Pitcher, N. G. Gwynne, H. S. O'Brien, S. W. Fill, H. T. Vane, Lord Cowdray, R. F. Rowbotham, Major-General W. B. Caddell, H. Knox, F. M. T. Lange, H. Noel, E. M. L. Wade, A. Boake, J. H. Newton, J. P. H. Bewsher, R. C. Searle, W. J. Skevington, H. G. Wells (90).

*Associate Members.*

J. S. Irving, J. W. Whitting, H. C. Newton, R. W. Potts, H. C. Rowland, Earl of Drogheda, R. A. W. Charlton, D. E. Cook, G. N. Ogilvie, H. T. Walker, Sir Archibald Denny, S. Barker, Sir Arthur du Cros, Lady Lawson, A. E. Owen, Captain C. F. Clapperton, Lady Burnham, Captain D. H. Kennedy, J. E. Hodgson, Muriel Wilson, Renée du Cros, W. E. King, W. H. White, Lieutenant W. S. Smith, Lieutenant A. E. Courage, L. D. Sloper, E. L. Gandar Dower, D. P. Muirhead, Lady Sarah Wilson, G. I. Price, D. M. Sullivan, A. Davidson, S. Holroyd, W. T. Havelock, Lady Jenkyns, W. Chater Lea, R. G. Laws, D. Mooney, J. N. P. Morris, F. J. Poynton, J. B. Phillipps, A. Freeland Squire, R. C. Carver, C. W. Mayne, Mrs. St. John E. Scott (47).

*Foreign Member.*

Colonel G. W. Dawes.

**Honours and Awards.**

Dr. R. T. Glazebrook, C.B., F.R.S., a Fellow of this Society and Director of the National Physical Laboratory, has received the honour of Knighthood.

**The Civil Aerial Transport Committee.**

Major-General R. M. Ruck, C.M.G., C.B., has been requested by the Air Board to act as the representative of this Society on the Civil Aerial Transport Committee.

# THE AIRCRAFT EXHIBITION "AT HOME."

On March 25th the Aëronautical Society of Great Britain, the Royal Aero Club, and the Society of British Aircraft Constructors held an "At Home" at the Aircraft Exhibition at the Grosvenor Galleries, by permission of the Countess of Drogheda, who organised the exhibition. The guests were received by the chairmen of the three societies, General Ruck, Professor Huntington and Mr. H. White Smith.

Colonel O'Gorman presided and briefly introduced the speakers.

## ADDRESS BY LORD SYDENHAM.

I have been asked to make some "general remarks," which, I think, means that I must not deal with scientific and highly technical matters. I wish to say first that all who have the interest of the air service at heart and all who are engaged in aeronautical questions owe a debt of gratitude to Lady Drogheda for organising this admirable exhibition. It will, I am sure, not only benefit the Flying Services Fund and the Irish Hospital Supply Depots, but help to diffuse knowledge and develop fresh and enthusiastic interest in all that concerns the conquest of the air. Beginning with Montgolfier, in 1783, it leads us by steps to the advent of the internal combustion engine, which was essential to the final triumph. The appliances, models, relics and photographs are extraordinarily interesting, and the illustrated catalogue is of permanent value. All this we owe to the hard work and great organising power of Lady Drogheda. When, as I hope, a great Institute of Aeronautics arises in London, it will no doubt have its museum containing a collection like this and carrying on the record of progress in the science of flying.

We are all good prophets after the event; but I do not think that any of us clearly foresaw the dominating position which the Air Service instantly assumed in the war. Perhaps we did not sufficiently realise to what the limited experience of the Italians in Tripoli pointed. Or we did not imagine the amazing progress made under the stimulus of necessity since August, 1914. It is always difficult in peace time to forecast the effect of new developments of warfare. This is inevitable because peace experiments never carry us far enough. It is easy now to see that, given vast armies with no great preponderance or marked difference of efficiency on either side, trench warfare must supervene if the first onslaught of the invader failed. We studied the campaign of 1870-71 more closely than the Russo-Japanese War, which was more instructive from the modern point of view. The unwieldiness of modern armies necessitates extensive fronts, and the delaying power of the rifle and machine gun makes out-flanking movements difficult. The leading characteristic of the war is thus explained and we can now see that it was inevitable.

Ever since the battle of the Marne, siege conditions have prevailed in the West, and from the moment they arrived, the gun and the aeroplane became dominating factors, and the former depended largely on the latter. Without heavy artillery in great masses, the attack of fortified positions is either hopeless or must involve immense sacrifice of life. Without the aeroplane, artillery is relatively ineffective. In combination, they have proved capable of wrecking the most formidable defences yet created. It can now be said with certainty that when artillery and air preponderance is available, the expenditure of sufficient shells will render the strongest works untenable or open to assault.

But the Air Service does more than this. By means of observation and photographs, it is now possible to provide the leaders of an attacking force with maps of the enemy's positions behind his front lines. When this information is

not available, an attacking force disordered by its efforts may find itself confronted by unexpected difficulties and compelled to make new dispositions under heavy fire. Finally, air reconnaissance has been able to reveal the coming of counter-attacks. You will have noticed that the other day four French aeroplanes went up near Verdun and carried out their duties in a strong blizzard, which was a very gallant act. So far as trench warfare is concerned, therefore, it is just to say that the Air Service is vital to success and that by its means thousands of lives can be spared.

This is only one aspect of work in the air. In every theatre of war, distant reconnaissance and bombing operations have played a very important part. If we and our Allies had possessed greater resources, bombing might have caused far greater damage to the enemy's resources and lines of communication. During the recent retirement our aeroplanes have rendered most valuable services, and if we see a general retreat, they may prove more formidable in pursuit than the best light cavalry. If we carry imagination far enough, it is easy to conceive that, if their air power was sufficient, the Allies might be able to force the evacuation of France and Belgium by its means alone. Wherever operations in the open field, unhampered by walls of fortifications occur, as now in Mesopotamia, air reconnaissance must be of first class importance. In the hands of a great General it will be a most powerful aid to victory, and it will in a sense affect strategy which we are accustomed to believe unchangeable by advances in armaments.

Turning to naval operations, I am not sure that the enemy did not derive more advantage from aircraft over the sea than our navy. We had not evolved a considered policy, and we were not, therefore, in a position to take all the measures open to us. Zeppelins have kept the North Sea under close observation, and we do not yet know all that this has meant. I have no doubt that air observation played a part in the battle of Jutland by giving the Germans information of the relative positions and movements of our battle cruiser squadron and Grand Fleet. It is also probable that Zeppelins have been used to assist the operations of submarines. We now know that the aeroplane or seaplane is more than a match for the Zeppelin and I do not see how the balance is to be redressed in the future. The time will come when the powers of observation of the Zeppelin, which depends on daylight, will be severely curtailed if not ended, and the great rigid airship may cease to be employed for war purposes. Meanwhile our Naval Air Service, including non-rigids, has done excellent work in helping to combat submarines and in detecting mines in near waters. More work of this nature may be possible in the future, and heavier than air craft will form an indispensable part of the equipment of fleets and squadrons. The Naval Service has also carried out some effective and very valuable bombing operations on land.

We may, I believe, now lay down the principle that mastery in the air will be vital to success in future wars. When the war is over, we shall have controversies as to what is meant by the "command of the air." Absolute command is unlikely to be obtained; but it will be possible to gain such mastery, moral and material, as will enable the duties of the Air Service to be carried out by one combatant while denying equal advantage to the other. The essential quality of air power, which will always differentiate it from all other arms, is that it can be rapidly concentrated for a specific purpose from points several hundred miles apart. Air fighting, developed by British and French airmen and copied by the Germans, has played a supremely important part in the war. The fighting machine will, therefore, occupy the foremost place and will undergo great further progress in speed, climb and handling, in armament and in convenience for attacking purposes.

Nearly 300 years ago, Bacon wrote:—"Let any Prince or State think soberly of his forces except his militia of natives be of good and valiant men." If we ever produce another Bacon, which I doubt, he will certainly say:—"Let any

Prince or State think soberly of his forces except his air service be in complete efficiency and readiness for war and his airmen be good and valiant men."

The rapid rise of aeronautics to a position of first class importance is all to the advantage of the British Empire if we know how to turn it to full account. There is something in the service of the air which appeals to the instincts and suits the aptitude of the British race. The gallantry of our pilots from home and overseas has been conspicuous in every theatre of war. They have never hesitated to fight against odds, and it is just to say that they and the pilots of our Allies have shown greater daring, enterprise and initiative than their antagonists. We know the heavy strain that is being thrown upon our airmen at the front at this moment, when the enemy during his retreat is making great efforts to curb their activities. We know also that we can trust them to do all that devoted heroism can accomplish, and we are sure that every effort is being made to keep them provided with effective machines.

On the technical side there should be nothing to fear in the future if only our best brains and immense manufacturing powers are brought into play by proper organisation. We possess ample scientific ability to enable us to solve problems present and to come. We have to bring science and technical skill into full and harmonious co-operation. That can be brought about only by a well conceived administrative and executive machine, dealing with the Air Service as a whole, which we do not at present possess.

The widespread interest which air matters have aroused is proved by the excellent work carried on by those bodies which have met this evening. We shall need a great institute of aeronautics which will keep the British Empire in the van of progress and will work in touch with our Allies.

There is one more matter to which I wish to refer. After the war, the civil use of the air is certain to develop on a large scale, and aircraft used for civil purposes will be differentiated from those intended for war. Speed, climb and manœuvring power will be of less account than other qualities. Just as the modern warship is totally unfit for purposes of commerce, so the war aircraft will not fulfil civil requirements. Large aeroplanes will be used, and standardisation will arise. It will be necessary to have a great experimental establishment for the practical trial and test of civil machines. The civil development of aircraft will undoubtedly assist in building up and maintaining the war air services which will be unified some day and on which the security of the Empire must in great measure depend.

General BRANCKER: My Lords, Ladies and Gentlemen,—I feel very much embarrassed at being called on to address so distinguished and so representative an audience at very short notice and with no time or energy left for the preparation of a fitting speech. This is really an historical occasion, for never before have the Royal Aero Club, the Aëronautical Society, and the Society of Aircraft Manufacturers met together under similar conditions. I must congratulate Lady Drogheda on her enterprise in arranging such a meeting, and can only hope that it may lead to similar meetings in the future. The exhibition which we have all seen deals principally with the past, and shows how after more than a century of very slow progress, and what must have appeared to the majority as a futile struggle, the human race is now pushing forward rapidly to attain the complete mastery of the air. During the early days, and even up to the beginning of the war, most people were either very incredulous or alternatively very optimistic regarding aviation. Many looked upon it as doomed to failure from the very start, and I have more than once heard it described as a wicked, dangerous, expensive, and useless innovation, which could never possibly have any material success, and even many educated soldiers could not realise what a very big factor it would prove in warfare. Man is very slow to believe in a new innovation, but when he does assimilate a new idea he often becomes fanatical in his belief, and this has

rather been the case with the optimists in aviation. Well, the war came and gave aviation its opportunity, and it has more than justified all considered expectations, even the most optimistic, and many of the erstwhile unbelievers are now crying out loudly for the impossible; for instance, the destruction of Berlin from the air, the capture of the Kaiser by aeroplane, the burning of the whole of the crops of Germany and Austria by means of aircraft, and the attainment of absolute command of the air. Well, if the war goes on for a few more years perhaps we shall accomplish all these objects, but meanwhile aviation on both sides, the Allies and German, in spite of criticism has not done badly, and the military dreamer of four years ago has seen all his dreams realised and sometimes outstripped, so far as the uses of aeroplanes are concerned.

Now I will try to describe to you very briefly and very roughly the history of the development of aviation since the beginning of the war and the situation as it exists at present.

The history of our struggle with Germany on the Western Front has been one of alternate ups and downs from the very start. At the outset, and in the early stages, we were outnumbered, but had the advantage of performance. At first no fighting in the air took place and all machines were devoted to reconnaissance pure and simple. Then we initiated fighting and our policy was to attack every German machine we met in the air, and there came a period when no German machine would stand up to one of ours, and people at home began to talk of our having attained the command of the air. Then came the Fokker, which by dint of surprise and advertisement, aided a good deal by the Press of this country, had a short lived but much talked of success, and the cry went up that we had lost command of the air. The next step in progress is represented by our aerial operations during the battle of the Somme. At the beginning of July, by means of a continuous and relentless offensive in the air, we completely dominated the enemy and for a considerable period scarcely a hostile aeroplane ventured over our lines. Again the command of the air was claimed for us, and actually we had obtained a greater superiority than could have been fairly expected. Now during that Somme battle, both sides learnt much concerning the enormous value of aviation to an army, and the Germans having suffered from its effects whilst we only reaped the fruits of success, probably realised its importance even more vividly than we did. In consequence, at the present moment they are exerting all their strength in an attempt to wrest our superiority from us, and so prevent us from observing their retirement and any concentration they may be bringing against us. They have not succeeded, but already the cry is going up in England that we have lost the command of the air.

Well, I think I must now just try to give you a very brief outline of how the R.F.C. works in the field. The aeroplanes at our disposal are divided very roughly into two classes—reconnaissance machine and the fighting machine. The reconnaissance machines carry out all the duties required by the commanders in the field—reconnaissance of the enemy's position, observation of our artillery fire, photography, and in some cases local bombing attacks. These machines do not pretend to fight, nor need they be very good performers, as some of the duties demanded from them entail flying slowly. The fighters, on the other hand, are used to prevent similar work being carried out by the enemy, to protect our reconnaissance machines, and bombing raids, and to attack and destroy as many of the enemy's fighting machines as they possibly can, and in some cases carry out long range reconnaissance. Practically all duties except photography and artillery reconnaissance are carried out by groups of machines, and aerial battles, which are beginning more and more to take the form of fleet actions, are of constant recurrence. It will be seen from this that the efficient work of the air service, so far as the army is concerned, depends on fighting in the air, and so far, in spite of all German efforts, we have continued to carry out all the reconnaissance work demanded. We are having hard and bitter fighting and heavy casualties

on both sides, and this is certain to go on for a considerable period, probably getting more intense as the weather improves and the days grow longer, but personally I have no doubt that in the end we shall prove our superiority at all events to the extent that we will always be able to carry out the work that the army demands.

To return to the future, I think it requires someone of more leisure and more activity of brain than myself to evolve the developments that may come about. One thing is clear to me, that is that the development of aviation means more to the British Empire than to any other nation, because as it develops it will inevitably, slowly and surely eliminate the protection which the channel affords us, and its development in connection with the navy has really only just started and is capable of boundless possibilities.

Defence in aerial matters is extraordinarily difficult, much more so than any other form of warfare. In war, the offensive always has a very great advantage over the defensive, because of its power to concentrate all available forces at one point and to strike, whilst the defensive must attempt to be more or less safe everywhere. This is intensified ten times over in aviation. It may be said that we have proved aerial defence is possible by bringing down Zeppelins, but my personal opinion is that the Zeppelin is only a passing phase and that when the attack is carried out by aeroplanes there will be a very different and more difficult question to solve. Our only safety in the future lies in strength, and we must be strong enough to so threaten our enemies that they dare not attack us.

Its value from a commercial point of view I believe in instinctively, but I must leave it to the members of the societies gathered here to-night to push that on when the time comes. One thing is certain, if we can only make aviation a real commercial success, the development of an Imperial Air Service for war purposes should be a comparatively easy task.

I think the value of this exhibition has been twofold:—

(1) As was intended, it has been the means of collecting a large sum of money for the benefit of those who are suffering in the fighting which is taking place at present, and

(2) Which was probably not intended originally, it has been the means of making many people of all classes think a little more about aviation and its wonderful possibilities in the future.

And before we can depend on a proper measure of expansion and development, when peace is declared we must have public opinion with us.

I think you will all join me in congratulating Lady Drogheda most heartily on the great success of this exhibition, and you will all agree with me that it is a fine example of what can be done by enthusiasm, energy, and personal initiative.

# ANNUAL GENERAL MEETING.

The Annual General Meeting of the Aëronautical Society of Great Britain was held on Wednesday, June 13th, 1917, at 7.0 p.m., at the Offices of the Society, 7, Albemarle Street, London, W. The Chairman of Council, Major-General R. M. Ruck, C.M.G., C.B., R.E., presided.

The Secretary read the notice convening the meeting.

The Chairman asked that the meeting approve the postponement until that evening, owing to the removal to the new offices, of the date of the Annual General Meeting, which should, under Rule 11, have been held before the 31st March.

The postponement was approved.

The annual report of the Council and accounts having been circulated were formally approved.

The Secretary read the proposals put forward by the Council for the alterations of the rules of the Society.

An amendment, moved by Mr. Cooper, seconded by Dr. Stanton and supported by Dr. Walmesley, was embodied in the Council's proposal and adopted *nem. con.*:—

> "That Rule 23 (*c*) shall read:—'Every Fellow of the Society shall be elected by the Council of the Society and the names of those elected shall be submitted for confirmation at the next ensuing general meeting"; and that Rule 23 (*d*) shall read:—'Every Associate Fellow of the Society shall be elected by the Council of the Society and the names of those elected shall be submitted for confirmation at the next ensuing general meeting.'"

It was recommended by the Council that the entrance fee of ten shillings charged to Student Members of the Society be remitted for the next ensuing twelve months.

The recommendation was unanimously adopted.

It was moved by Mr. Cooper, seconded by Dr. Stanton, and unanimously resolved that consideration be deferred of the recommendations of the Council contained in the agenda paper as follows:—

1. That the grade known as "Associate Fellow" be in future known by the name "Member."

2. That the grade now known by the name "Member" be in future known by the name "Associate."

3. That the grade now known by the name "Associate Member" be in future abolished, and that the present members thereof be transferred to the "Associate" grade.

Together with the further proposals dependent upon these clauses.

The Chairman explained that the Council's recommendations had been very fully discussed in Council and were supported by very serious arguments. There was, however, no desire to force them upon the Society without the fullest possible discussion. The step contemplated was one of considerable gravity and he was

certain that the meeting had taken the right course under all the circumstances. There had been some inquiry among the voters of the Society as to the precise effect of the changes suggested, many of whom were unable to be present there that night, and he personally welcomed the opportunity now afforded them of further information.

Messrs. Griffith Brewer, C. S. Turner and Turnbull were appointed scrutineers of the ballot for the Council, and on a count of the votes received the following were declared elected:—Lieut.-Col. O'Gorman, C.B., Mr. A. E. Berriman, Mr. F. Handley Page, Mr. G. Holt Thomas, Comdr. W. Briggs, Comdr. Alec Ogilvie, Lieut. A. P. Thurston, and Mr. B. G. Cooper.

The meeting then terminated.

# THE AERONAUTICAL SOCIETY OF GREAT BRITAIN.

## Council's Report, 1916-17.

The past year has been one of considerable development and progress in the affairs of the Society, more especially in respect to the large increase in membership, the resumption of the full activities of the Society, the establishment of a Joint Standing Committee with the Society of British Aircraft Constructors, and the transfer of the offices from Adam Street to much more suitable and commodious premises in Albemarle Street.

It is evident that there is now a much more general interest taken in aeronautical matters than formerly, and the war has, of course, increased enormously the manufacture of aircraft and hence the numbers of those qualified for the technical membership of the Society. The Council have endeavoured to take every advantage of this movement for the benefit of the Society, and hope that the following account of their activities will meet with the approval of the members :—

### Further Development.

The Council has given a great deal of attention to the future development of the Society and its position in relation to other societies and institutions of a kindred nature, and has held a Joint Conference with the Council of one of these Societies, the Institute of Automobile Engineers. They have further sought and received much valuable advice and assistance from various distinguished members of the various Societies who attended the meetings of Council by invitation.

Thus the Council has debated the question whether any alteration or addition to the name of the Society is desirable, and has decided in the negative.

The Council discussed at considerable length the question of the grades of membership, the rules for which were drawn up in 1911, when technical knowledge of aeronautics was decidedly limited and confined to a very few individuals. Circumstances have altered largely since that date, as have the opinions held as to the constitution and organisation of Scientific and Engineering Societies and Institutions.

After careful discussion, and with the assistance of various experts on the subject, the Council have recommended to the Society the alteration in Rules shown on the official notice circulated to the voters of the Society.

### Relations with the Society of British Aircraft Constructors.

After a series of meetings last autumn the Council of this Society and the S.B.A.C. agreed to form a Joint Standing Committee of Relations for the general benefit of aeronautical development, it being mutually recognised that the Aëronautical Society are the paramount authority in aeronautical science, whilst the S.B.A.C. represent the associated aircraft manufacturers of the British Empire. The detailed provisions of the arrangement have already been published in the Society's Journal. Since the date of its formation the Committee of Relations has met frequently and has discussed questions affecting the two Societies. It is at present engaged in drawing up a statement on the commercial development of

aircraft after the war, being advised on the technical side by a Special Research Committee of the Aëronautical Society. It may be observed that since the Committee commenced the investigation just referred to the Government have approved a Committee to deal with the same subject. The Council are in touch with the Air Board on this matter, and steps have been taken to avoid overlapping. The Standing Committee of Relations has also conferred with the Department of Scientific and Industrial Research on several occasions with a view to establishing an approved organisation for aeronautical research, in which the Aëronautical Society and the S.B.A.C. shall participate. Negotiations are proceeding, and it is hoped that before long a satisfactory scheme will be adopted.

The Society offer their acknowledgments to the Society of British Aircraft Constructors for the great interest they have taken in the scientific aspects of the industry they represent and for the support they have given to our Society.

**Meetings.**

A very full and interesting course of lectures was arranged for the first half of the current year, and special thanks are due to Lieutenant-Colonel O'Gorman, C.B., who was largely instrumental in arranging the syllabus. The Council desire to acknowledge the kindness of Admiral G. N. Paine, C.B., R.N. (Fifth Sea Lord), Mr. F. W. Lanchester, M.Inst.C.E., Mr. Henry Fowler (Superintendent, Royal Aircraft Factory), Brigadier-General W. S. Brancker, R.A., Lord Sydenham, F.R.S., G.C.I.E., Professor A. N. Whitehead, F.R.S., Colonel Sir Capel Holden, K.C.B., F.R.S., Sir Charles Parsons, K.C.B., D.Sc., F.R.S., Major Lyons, F.R.S., Mr. Dugald Clerk, D.Sc., F.R.S., Lord Cowdray (President of the Air Board), and Lieutenant-General Sir David Henderson (Director-General of Military Aeronautics) for their kindness and support in consenting to preside at the fortnightly meetings.

The Council also offer their most cordial thanks to Brigadier-General Brancker, Mr. F. Handley Page, Captain G. S. Walpole, Major G. I. Taylor, Captain H. J. Tizard, Mr. M. A. S. Riach, Mr. A. B. Young, Dr. W. H. Hatfield, Captain C. P. Cave, Mr. L. Coatalen, Mr. G. Holt Thomas, Captain B. C. Hucks, Dr. R. Mullineux Walmsley, and Mr. C. E. Larard for their services in reading before the Society the series of admirable lectures which have distinguished the 1917 Session of the Society's work.

**Educational Lectures.**

Full courses of lectures of an educational character have been delivered at Cardiff and at Hendon.

The Cardiff course was arranged in association with the Cardiff Technical Instruction Committee, and its initiation was due to the public spirit of Alderman David, who defrayed the expenses.

The Hendon course was held in St. Peter's Hall, Cricklewood Lane, N.W., by kind permission of Mr. Handley Page, who also defrayed the cost of the lectures.

The thanks of the Society are also due to the Advisory Committee for Aeronautics for the permission given for members of the Staff of the National Physical Laboratory to give lectures. The Council confess their great indebtedness in respect of these educational lectures to Mr. B. G. Cooper, Mr. E. S. Relf, Mr. A. Fage, Captain F. S. Barnwell, Mr. R. O. Boswall, Lieutenant-Colonel Waterlow, Mr. L. Bairstow, F.R.S., and Lieutenant J. S. Irving, who

delivered the lectures, and to Mr. Handley Page, Captain de Havilland, Mr. Holt Thomas, Mr. Griffith Brewer, Colonel O'Gorman, and Mr. Hugh Burroughes for so kindly presiding at them.

**New Premises.**

It had been fully realised for a long time past that the rooms of the Society in Adam Street were totally inadequate for the Society, but the funds of the Society did not justify the expenditure necessary for adequate accommodation. Owing, however, to careful expenditure in the past and to a considerable increase in membership, the Council decided early in the year that the Society could now afford suitable premises, and they were fortunate enough to obtain a lease of the offices in 7, Albemarle Street on very favourable terms. The new premises include excellent offices for the staff of the Society and a comfortable and spacious reading-room for members. Thanks to the generosity of some of the members, the entire cost of the transfer and the equipment of the offices has been defrayed without touching the funds of the Society.

Special mention should be made of the great generosity of one of the Society's oldest and most valuable members, Mr. Patrick Alexander. The grateful thanks of the Society are also due to Mr. Holt Thomas for a gift of £150 towards furnishing the new offices, and also for the presents of furniture which have been received from Lieutenant-Colonel O'Gorman and Messrs. Griffith Brewer, Cooper, and Major-General Ruck, and of prints from Mr. Barnard Faraday.

**Secretaryship.**

In December, 1916, Mr. Cooper resigned the Secretaryship of the Society, and the Council advertised for a new Secretary, with the result that out of 350 applications Mr. W. Barnard Faraday, LL.B., F.S.S., Barrister-at-Law, was selected to fill the appointment. Miss Phyllis Boyd was at the same time appointed Assistant Secretary. The Council desire to take the opportunity of acknowledging the very valuable services rendered by Mr. Cooper during his term of office.

**Library.**

A considerable number of books and pamphlets have been added to the Library during the year in addition to the usual periodicals, and the Council desire to acknowledge with thanks the kindness of the donors. The Library is undergoing reorganisation. The Council have in view a lending department as well as the reference side of the Library.

**Wilbur Wright Memorial Lecture.**

The fifth Wilbur Wright Memorial Lecture will be read on June 13th, 1917, by Lieutenant-Colonel M. O'Gorman, C.B.

**Popular Lecture.**

The Council, thinking the occasion a suitable one, decided to afford the large public interested in the development of the use of aircraft an opportunity of hearing Mr. Holt Thomas on May 30th. The result was a striking success. An audience to the number of over 2,500, under the presidency of Lord Cowdray,

assembled at the Central Hall, Westminster, and the Council have received since many gratifying expressions of appreciation from those who attended.

**Finance.**

The financial situation of the Society is satisfactory, but in view of the heavy responsibilities which the Society has assumed, further progress is necessary. Up to Christmas, 1916, the following were the additions to the Society's membership since the last annual meeting:—Members, 11; Associate Members, 7; Students, 4. Since then the Society's active programme has begun, and with the opening of 1917 a great influx of members has taken place, the figures for the last five months being:—Members, 85; Associate Members, 39; Students, 17. In addition, the Society is now being asked to ballot for three new Fellows and 46 Associate Fellows. The credit balance of the Society on December 31st, 1916, including unpaid subscriptions, was £704 9s. 4d., the excess of income over expenditure for the year being £233 2s. 4d. For the information of those concerned, it may be observed that at the present time the Funds of the Society (apart from the various Trust Funds which the Society administers) stand at over £1,750, while for the past five months the excess of income over expenditure has been approximately £1,000. The past five months, therefore, have been by far the most prosperous in the Society's history, and the Society is now in a position to invest practically the whole of the sums received as entrance fees and life compositions, in accordance with the view of the Hon. Auditor that these moneys should be treated as capital and not as income.

The Council draw attention to the very serious unpunctuality in the payment of subscriptions. Subscriptions in default at the end of 1916 amounted to £226 16s. The amount owing to the Society has been very considerably increased by delay in the payment of the current subscriptions for the present year. Most of these subscriptions will, it is expected, be paid in course of time, but the Council beg to point out that inconvenience and additional labour are thrown on the office staff by this irregularity, while the future of the Society is prejudiced by uncertainty as to its financial condition.

## AERIAL
## (The Aeronautical

GENERAL INCOME AND EXPENDITURE ACCOUNT,

EXPENDITURE.

| | £ s. d. | £ s. d. |
|---|---|---|
| To Office Rental, Lighting and Insurance ... ... | 104 12 6 | |
| ,, Salaries ... ... ... ... ... ... | 199 9 8 | |
| ,, Exhibitions and General Meetings ... ... ... | 25 4 0 | |
| ,, Printing, Stationery, etc. ... ... ... ... | 10 12 11 | |
| ,, Postages, Messengers, etc. ... ... ... ... | 19 11 6 | |
| ,, Library Expenditure, less Income ... ... ... | 12 12 3 | |
| ,, Office Expenses ... ... ... ... ... | 34 3 5 | |
| | | 406 6 3 |
| ,, Balance being excess of Income over Expenditure ... ... | | 233 2 4 |
| | | £639 8 7 |

BALANCE SHEET,

LIABILITIES.

| | £ s. d. | £ s. d. |
|---|---|---|
| Nominal Capital, divided into 20 Shares of 1s. each and 999 Shares of £1 each ... ... ... | 1,000 0 0 | |
| Capital issued and called up:— | | |
| 11 Shares issued and fully paid up ... ... | | 0 11 0 |
| Sundry Creditors ... ... ... ... ... ... | | 4 0 10 |
| Subscriptions paid in advance ... ... ... ... | | 30 17 4 |
| Revenue Account:— | | |
| Surplus 1915 balance ... ... ... ... | 471 7 0 | |
| Excess of Income over Expenditure, 1916 ... | 233 2 4 | |
| | | 704 9 4 |
| | | £739 18 6 |

I beg to report that all my requirements have been complied with by the Balance Sheet is properly drawn up so as to exhibit a true and correct view of the explanations given to me, and as shown by the books of the Company. Income, are included in the revenue.

## SCIENCE LTD.

### Society of Great Britain).

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31ST DECEMBER, 1915, TO 31ST DECEMBER, 1916.

INCOME.

| | £ | s. | d. |
|---|---|---|---|
| By Annual Subscriptions ... ... ... ... ... ... | 399 | 13 | 6 |
| ,, Life Members ... ... ... ... ... ... ... ... | 26 | 5 | 0 |
| ,, Entrance Fees ... ... ... ... ... ... ... | 52 | 8 | 0 |
| ,, Interest on Deposit Account... ... ... ... ... ... | 1 | 17 | 3 |
| ,, Grant from Army Funds ... ... ... ... ... ... | 87 | 0 | 0 |
| ,, Journals, Pamphlets, etc., balance ... ... ... ... ... | 72 | 4 | 10 |
| | £639 | 8 | 7 |

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31ST DECEMBER, 1916.

ASSETS.

| | £ | s. | d. |
|---|---|---|---|
| Cash at Bank—Coutts and Co. ... ... ... ... ... ... | 383 | 14 | 3 |
| Cash in hand ... ... ... ... ... ... ... ... | 5 | 7 | 7 |
| Office Furniture, Aeroplanes, Printed Books, Stationery, Old Prints, etc., as per last Balance Sheet ... ... ... ... | 88 | 0 | 0 |
| Stock of Journals, Blocks, etc., at Society and with the Society's Printers ... ... ... ... ... ... ... ... | 10 | 0 | 0 |
| Sundry Debtors, including Subscriptions owing ... ... ... | 227 | 17 | 0 |
| Reserves for amounts paid in advance ... ... ... ... ... | 24 | 19 | 8 |
| | £739 | 18 | 6 |

Directors and Officers of the Company, and that in my opinion the foregoing the state of the Company's affairs, according to the best of my information and Entrance Fees which, strictly speaking, should not be looked upon as Annual

(Signed) JAMES E. WARD,
Chartered Accountant,
London and Birmingham,
*Honorary Auditor.*

# METHODS OF MEASURING AIRCRAFT PERFORMANCES.

BY CAPTAIN H. T. TIZARD, R.F.C.

## AEROPLANE TESTING.

The accurate testing of aeroplanes is one of the many branches of aeronautics which have been greatly developed during the war, and especially during the last year. For some months after the war began a climb to 3,000 to 5,000 feet by aneroid and a run over a speed course was considered quite a sufficient test of a new aeroplane; now we all realise that for military reasons certainly, and probably for commercial reasons in the future, it is the performance of a machine at far greater heights with which we are mainly concerned. In this paper I propose to give a short general account of some of the methods of testing now in use at the Testing Squadron of the Royal Flying Corps, and to indicate the way in which results of actual tests may be reduced, so as to represent as accurately as possible the performance of a machine independently of abnormal weather conditions, and of the time of the year. For obvious reasons full details of the tests and methods employed cannot yet be given. So far as England is concerned, I believe that the general principles of what may be called the scientific testing of aeroplanes were first laid down at the Royal Aircraft Factory. Our methods of reduction were based on theirs to a considerable extent, with modifications that were agreed upon between us; they have been still further modified since, and recently a joint discussion of the points at issue has led to the naval and military tests being co-ordinated, so that all official tests are now reduced to the same standard. It should be emphasised that once the methods are thought out scientific testing does not really demand any high degree of scientific knowledge; in the end the accuracy of the results really depends upon the flyer, who must be prepared to exercise a care and patience unnecessary in ordinary flying. Get careful flyers whose judgment and reliability you can trust and your task is comparatively easy; get careless flyers and it is impossible.

At the outset it may be useful to point out by an example the nature of the problems that arise in aeroplane testing. Suppose that it is desired to find out which of two wing sections is most suitable for a given aeroplane. The aeroplane is tested with one set of wings, which are then replaced by the other set, and the tests repeated some days later. The results might be expressed thus:—

| | A Wings. | B Wings. |
|---|---|---|
| Speed at 10,000ft. ... ... | 90 m.p.h. | 93 m.p.h. |
| Rate of climb at 10,000ft. ... | 250ft. a minute. | 300ft. a minute. |

Now the intelligent designer knows, or soon will know, that firstly an aneroid may indicate extremely misleading "heights," and secondly, that even if the actual height above the ground is the same in the two tests the actual conditions of atmospheric pressure and temperature may have been very different on the two days. He will therefore say, what does that 10,000 mean? Do you mean that your aneroid read 10,000 feet, or do you mean 10,000 feet above the spot you started from, or 10,000 feet above sea level? If he proceeds to think a

trifle further he will say—what was the *density* of the atmosphere at your 10,000 feet; was it the *same* in the two tests? If not, the results do not convey much. There he will touch the keynote of the whole problem, for it is on the density of the atmosphere that the whole performance of an aeroplane depends; the power of the engine and the efficiency of the machine depend essentially on the density, the resistance to the motion of the machine through the air is proportional to the density, and so finally is the lift on the wings. None of these properties are proportional solely to the pressure of the atmosphere, but to the density, that is the weight of air actually present in unit volume. It follows that it is essential when comparing the performances of machines to compare them as far as possible under the same conditions of atmospheric *density*, not as is loosely done at the same height above the earth, since the density of the atmosphere at the same height above the earth may vary considerably on different days, and on the same day at different places.

At the same time, in expressing the final results, this principle may be carried too far. Thus, if the speed of a machine were expressed as 40 metres a second at a density of 0.8 kilogrammes per cubic metre, the statement, though it may be strictly and scientifically accurate, will convey nothing to 99 per cent. of those directly concerned with the results of the test. The result is rendered intelligible and indeed useful by the form "90 m.p.h. at 10,000 feet," or whatever it is. With this form of statement, in order that all the statements of results may be consistent and comparative, we must be careful to mean by "10,000 feet" a certain definite density, in fact the average density of the atmosphere at a height of 10,000 feet above mean sea level. This is what the problem of "reduction" of tests boils down to; what is the relation between atmospheric density and height above sea level? This knowledge is obtained from meteorological observations. We have collected all the available data—mostly unpublished—with results shown in the following table:—

TABLE I.

MEAN ATMOSPHERIC PRESSURE, TEMPERATURE AND DENSITY AT VARIOUS HEIGHTS ABOVE SEA LEVEL.

| Height in kilometres. | Height in equivalent feet. | Mean pressure in millibars. | Mean temp. in absolute degrees centigrades. | Mean density in kgm. per cubic metre. |
|---|---|---|---|---|
| 0 | 0 | 1,014 | 282 | 1.253 |
| 1 | 3,280 | 900 | 278 | 1.128 |
| 2 | 6,560 | 795 | 273 | 1.014 |
| 3 | 9,840 | 699 | 268 | 0.909 |
| 4 | 13,120 | 615 | 262 | 0.818 |
| 5 | 16,400 | 568 | 255 | 0.735 |
| 6 | 19,680 | 469 | 248 | 0.658 |
| 7 | 22,960 | 407 | 241 | 0.589 |

These are the mean results of a long series of actual observations made by Mr. W. H. Dines, F.R.S. It is convenient to choose some density as standard, call it unity, and refer all other densities as fractions or percentages of this "standard density." We have taken, in conformity with the R.A.F., the density of dry air at 760m.m. pressure and 16° centigrade as our *standard* density; it is 1.221kgm. per cubic metre. The reason this standard has been taken is that the air speed indicators in use are so constructed as to read correctly at this density, assuring the law:—

$$p = \tfrac{1}{2}\rho V^2$$

where $V$ is the air speed, $p$ the pressure obtained, $\rho$ the standard density.

In some ways it would doubtless be more convenient to take the average density at sea level as the standard density, but it does not really matter what you take so long as you make your units quite clear. Translated into feet, and fraction of the standard density the above table becomes:—

TABLE II.

| Height in feet. | Percentage of standard density. |
|---|---|
| 0 | 102.6 |
| 1,000 | 99.4 |
| 2,000 | 96.3 |
| 3,000 | 93.2 |
| 4,000 | 90.3 |
| 5,000 | 87.4 |
| 6,000 | 84.6 |
| *6,500 | 83.3 |
| 7,000 | 81.9 |
| 8,000 | 79.2 |
| 9,000 | 76.5 |
| *10,000 | 74.0 |
| 11,000 | 71.7 |
| 12,000 | 69.5 |
| *13,000 | 67.3 |
| 14,000 | 65.2 |
| 15,000 | 63.0 |
| 16,000 | 61.1 |
| *16,500 | 60.1 |
| 17,000 | 59.1 |
| 18,000 | 57.1 |
| 19,000 | 55.2 |
| 20,000 | 53.3 |

Let us briefly consider what these figures mean. For example, we say that the density at 10,000 feet is 74 per cent. of our standard density, but it is not meant that at 10,000 feet above mean sea level the atmospheric density will always be 74 per cent. of the standard density. Unfortunately for aeroplane tests this is far from true. The atmospheric density at any particular height may vary considerably from season to season, from day to day, and even from hour to hour; what we do mean is that if the density at 10,000 feet could be measured every day, then the *average* of the results would be, as closely as we can tell at present, 74 per cent. of the standard density.

The above table may therefore be taken to represent the conditions prevailing in a " normal " or " standard " atmosphere, and we endeavour, in order to obtain a strict basis of comparison, to reduce all observed aeroplane performances to this standard atmosphere, *i.e.*, to express the final results as the performance which may be expected of the aeroplane on a day on which the atmospheric density at every point is equal to the average density at the point. Some days the aeroplane may put up a better performance, some days a worse, but on the average, if the engine power and other characteristics of the aeroplane remain the same, its performance will be that given.

It must be remembered that a standard atmosphere is a very abnormal

* 6,500 feet is introduced as corresponding roughly to the French test height of 2,000 metres. 10,000 feet similarly corresponds roughly to comparing aeroplane test performances to the French standard of 3,000 metres, and similarly for 13,000 and 16,500 feet.

occurrence; besides changes in density there may occur up and down air currents which exaggerate or diminish the performance of an aeroplane, and which must be taken carefully into account. They show themselves in an otherwise unaccountable increase or decrease in rate of climb or in full speed flying level at a particular height.

We now pass to the actual tests, beginning with a description of the observations which have to be made and thereafter to the instruments necessary. The tests resolve themselves mainly into

(*a*) A climbing test at the maximum rate of climb for the machine.

(*b*) Speed tests at various heights from the "ground" or some other agreed low level upwards.

Experience agrees with theory in showing that the best climb is obtained by keeping that which is frequently called the air speed of an aeroplane, namely, the indications of the ordinary air speed indicator, nearly constant whatever the height. [In other words $\rho V^2$ is kept constant.] We can look at this in this way. There is a limiting height for every aeroplane above which it cannot climb; at this limiting height, called the ceiling of the machine, there is only one speed at which the aeroplane will fly level, at any other air speed higher or lower it will descend. Suppose this speed be 55 m.p.h. on the air speed indicator. Then the best rate of climb from the ground is obtained by keeping the speed of the machine to a steady indicated 55 m.p.h. Fortunately a variation in the speed does not make very much difference to the rate of climb; for instance, a B.E.2c with a maximum rate of climb at 53 m.p.h. climbs just as fast up, say, to 5,000 feet at about 58 m.p.h. This is fortunate as it requires considerable concentration to keep climbing at a steady air speed, especially with a light scout machine; if the air is at all "bumpy" it is impossible. At great heights the air is usually very steady, and it is much easier to keep to one air speed. It is often difficult to judge the best climbing speed of a new machine; flyers differ very much on this point, as on most. The Testing Squadron, therefore, introduced some time ago a rate of climb indicator intended to show the pilot when he is climbing at the maximum rate. It consists of a thermos flask, communicating with the outer air through a thermometer tube leak. A liquid pressure gauge of small bore indicates the difference of pressure between the inside and outside of the vessel. Now, when climbing, the atmospheric pressure is diminishing steadily; the pressure inside the thermos flask tends therefore to become greater than the outside atmospheric pressure. It goes on increasing until air is being forced out through the thermometer tubing at such a rate that the rate of change of pressure inside the flask is equal to the rate of change of atmospheric pressure due to climbing. When climbing at a maximum rate, therefore, the pressure inside the thermos flask is a maximum. The pilot therefore varies his air speed until the liquid in the gauge is as high as possible, and this is the best climbing speed for the machine.

What observations during the test are necessary in order that the results may be reduced to the standard atmosphere? Firstly, we want the time from the start read at intervals, and the height reached noted at the same time. Here we encounter a difficulty at once, for there is no instrument which records height with accuracy. The aneroid is an old friend now of aeronauts as well as of mountaineers, but although it has often been tentatively exposed, it is doubtful whether 1 per cent. of those who use it daily realise how extraordinarily rare it is that it ever does what it is supposed to do, that is, indicate the correct height above the ground, or starting place. The faults of the aeroplane aneroid are partly unavoidable and partly due to those who first laid down the conditions of its manufacture. An aneroid is an instrument which in the first place measures only the *pressure* of the surrounding air. Now if $p_1$ and $p_2$ are the pressures at

two points in the atmosphere, the difference of height between these points is given very closely by the relation,

$$h = k\theta \log_e {}^{p1}/_{p2}$$

where $\theta$ is the average temperature, expressed in "absolute" degrees, of the air between the two points and $k$ is a constant. It is obvious that if we wish to graduate an aneroid in feet we must choose arbitrarily some value for $\theta$. The temperature that was originally chosen for aeroplane aneroids was 50 degrees Fahrenheit or 10 degrees centigrade. An aneroid, as now graduated, will therefore only read the correct height in feet if the atmosphere has a uniform temperature of 50 degrees Fahrenheit from the ground upwards, and it will be the more inaccurate the greater the average temperature between the ground and the height reached differs from 50° Fahr. Unfortunately 50° F. is much too high an average temperature; to take an extreme example it is only on the hottest days in summer, and even then very rarely, that the average temperature between the ground and 20,000 feet will be as high as 50° F. On these very rare occasions an aneroid will read approximately correctly at high altitudes; otherwise it will always read too high. In winter it may read on cold days 2,000 feet too high at 16,000 feet, *i.e.*, it will indicate a height of 18,000 feet when the real height is only 16,000 feet. It is always necessary therefore to "correct" the aneroid readings for temperature. The equation

$$H = \frac{273 + t}{283} \cdot h \ .$$

gives us the necessary correction. Here $H$ is the true difference in height between any two points, $t$ the average temperature in degrees centigrade between the points, and $h$ the difference in height indicated by aneroid. It is convenient to draw a curve showing the necessary correction factors at different temperatures, some of which are given in the following table:—

TABLE III.

ANEROID CORRECTION FACTORS.

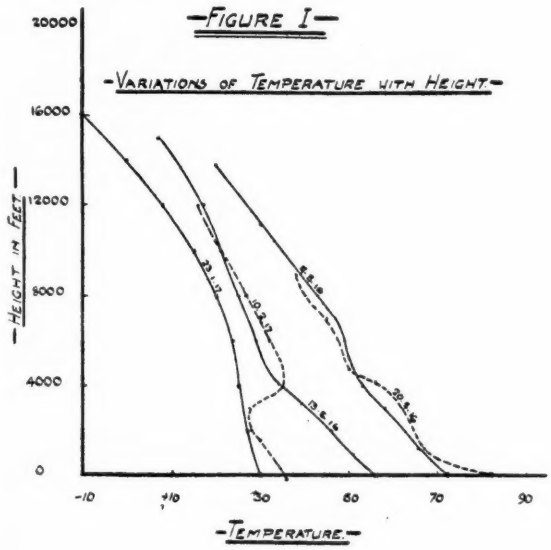
| Temperature. | Correction factor. |
|---|---|
| 70° Fahr. | 1.040 |
| 50° ,, | 1.000 |
| 30° ,, | 0.961 |
| 10° ,, | 0.922 |
| — 10° ,, | 0.883 |

For example, if a climb is made through 1,000 feet by aneroid and the average temperature is 10° Fahr., the actual distance in feet is only $1{,}000 \times 0.922 = 922$ feet. The above equation is probably quite accurate enough for small differences of height—up to 1,000 feet say—and approximately so for bigger differences.

The magnitude of the correction which may be necessary shows how important it is that observations of temperature should be made during every test. For this purpose a special thermometer is attached to a strut of the machine, well away from the fuselage, and so clear of any warm air which may come from the engine. The French, I believe, do not measure temperature, but note the ground temperature at the start of a test, and assume a uniform fall of temperature with height. This, undoubtedly, may lead to serious errors. The change of temperature with height is usually very irregular, and only becomes fairly regular at heights well above 10,000 feet. This is shown by the following curves,

which represent results taken at random from tests made at different times (Fig. 1).

The aneroid being what it is, one soon comes to the conclusion that the only way to make use of it in aeroplane tests is to treat it purely as a pressure instrument. For this reason it is best to do away with the zero adjustment for all test purposes and lock the instrument so that the zero point on the height scale corresponds to the standard atmospheric pressure of 29.9 inches or 760m.m of mercury. Every other height then corresponds to a definite pressure; for instance, the locked aneroid reads 5,000 feet when the atmospheric pressure is 24.88ins., and 10,000 feet when it is 20.70ins., and so on. If the temperature is noted at the same time as the aneroid reading, we then know both the atmospheric pressure and temperature at the point, and hence the density can be calculated, or, more conveniently, read off curves drawn for the purpose. The observations




necessary (after noting the gross aeroplane weight, and nett or useful weight carried) are therefore, (i.) aneroid height every 1,000 feet, (ii.) time which has elapsed from the start of the climb, and (iii.) temperature; to these should be added also (iv.) the air speed and (v.) engine revolutions at frequent intervals. The observed times are then plotted on squared paper against the aneroid heights and a curve drawn through them. From this curve the rate of climb at any part (also in aneroid feet) can be obtained by measuring the tangent to the curve at the point. This is done for every 1,000 feet by aneroid. The *true* rate of climb is then obtained by multiplying the aneroid rate by the correction factor corresponding to the observed temperature. These true rates are then plotted afresh against standard heights and from this curve we can obtain the rates of climb corresponding to the standard heights 1,000, 2,000, 3,000, etc. Knowing the change of rate of climb with height, the time to any required height is best obtained by graphical integration. The following table gives the results of an actual test:—

## TABLE IV.

Machine..... Engine.....

Date 27/12/16.

| Height in Aneroid ft. | Observed temp. | Percentage of standard density. | Observed time. | Rate of climb in Aneroid ft. | Real rate of climb (corrected for temp.) | From Curve. Standard height | From Curve. % of standard density. | From Curve. Time. | From Curve. Rate of climb. |
|---|---|---|---|---|---|---|---|---|---|
| 0 | 36° Fahr. | | 0.0 | | | | | | |
| 1,000 | 37° ,, | 101.0 | 1.0 | 835 | 814 | 1,000 | 99.40 | 1.20 | 775 |
| 2,000 | 38° ,, | 97.2 | 2.10 | 735 | 718 | 2,000 | 96.30 | 2.56 | 685 |
| 3,000 | 36° ,, | 94.0 | 3.70 | 640 | 622 | 3,000 | 93.26 | 4.11 | 610 |
| 4,000 | 36° ,, | 90.7 | 5.40 | 560 | 544 | 4,000 | 90.25 | 5.85 | 545 |
| 5,000 | 36° ,, | 87.4 | 7.25 | 510 | 495 | 5,000 | 87.35 | 7.80 | 490 |
| 6,000 | 33° ,, | 84.7 | 9.40 | 450 | 435 | 6,000 | 84.50 | 9.96 | 435 |
| 7,000 | 30° ,, | 82.1 | 11.90 | 405 | 389 | 7,000 | 81.80 | 12.40 | 385 |
| 8,000 | 26° ,, | 79.9 | 14.25 | 365 | 347 | 8,000 | 79.16 | 15.14 | 345 |
| 9,000 | 22° ,, | 77.6 | 17.00 | 330 | 312 | 9,000 | 76.55 | 18.20 | 310 |
| 10,000 | 23° ,, | 74.7 | 20.25 | 310 | 294 | 10,000 | 74.00 | 21.61 | 280 |
| 11,000 | 21° ,, | 72.2 | 23.60 | 280 | 264 | 11,000 | 71.70 | 25.41 | 245 |
| 12,000 | 20° ,, | 69.8 | 27.40 | 230 | 216 | 12,000 | 69.50 | 29.81 | 210 |
| 13,000 | 17° ,, | 67.7 | 31.90 | 195 | 182 | 13,000 | 67.32 | 35.13 | 170 |
| 14,000 | 12° ,, | 65.9 | 37.90 | 150 | 139 | 14,000 | 65.17 | 41.88 | 130 |
| 15,000 | 8° ,, | 64.1 | 45.25 | 110 | 101 | 14,500 | 64.11 | 46.23 | 105 |

The corrections are often much greater than those necessary in the above case.

It will be noticed that the rate of climb of this machine is approximately halved for a difference in height of 5,000 feet. Now it is possible to get a difference in density near the ground of as much as 15 per cent. between a hot day in summer and a cold day in winter. This corresponds to a difference in height of 5,000 feet (see Table II.), so that this machine would climb off the ground on a hot day at only half the rate that it would on a very cold day. Variation in atmospheric density, combined with the errors of an aneroid, fully account for the observed difference between a "good climbing day" and a "bad climbing day."

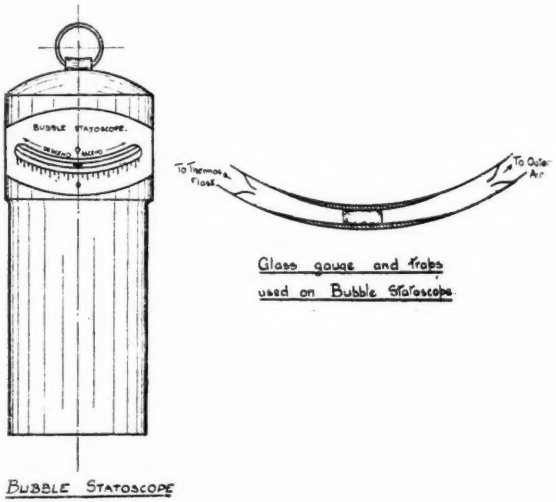
At least two climbing tests of every new machine are carried out up to 16,000 feet or over by aneroid. If time permits three or more tests are made. The final results given are the average of the tests and represent as closely as possible the performance on a standard day, with temperature effects, up and down currents and other errors eliminated.

If we produce the rate of climb curve upwards it cuts the height axis at a point at which the rate of climb would be zero, and therefore the limit of climb reached. This is the "ceiling" of the machine.

## SPEEDS.

His 16,000 feet, or whatever it is, reached, the flyer's next duty is to measure the speed flying level by air speed indicator at regular intervals of height (generally every 2,000 feet) from the highest point downwards. To do this he requires a sensitive instrument which will tell him when he is flying level. The aneroid is quite useless for this purpose, and a "statoscope" is used. The principle of this instrument is really the same as that of a climbmeter. It consists of a thermos flask connected to a small glass gauge, slightly curved, but placed about horizontally. In this gauge is a small drop of liquid, and at either end

FIGURE 2




Glass gauge and traps used on Bubble Statoscope

BUBBLE STATOSCOPE

are two glass traps which prevent the liquid from escaping either into the outside air or into the thermos flask. As the machine ascends the atmospheric pressure becomes smaller, and the pressure in the flask being then higher than the external pressure, the liquid is pushed up to the right hand trap, where it breaks, allowing the air to escape. On descending the reverse happens; the liquid travels to the left, breaks, and air enters the flask. When flying truly level the drop remains stationary, moving neither up nor down. The instrument is made by the British Wright Co.

The flyer or the observer notes the maximum speed by the air speed indicator, *i.e.*, the speed at full engine throttle. At one or more heights also, he observes the speeds at various positions of the throttle down to the minimum speed which will keep the machine flying at the height in question. The petrol consumption and the engine revolutions are noted at the same time, as well, of course, as the aneroid height and temperature. Accurate observation of speeds needs very careful flying—in fact much more so than in climbing tests. If the air is at all

bumpy observations are necessarily subject to much greater error, since the machine is always accelerating and decelerating. The best way to carry out the test seems to be as follows. The machine is flown first just down hill and then just up hill and the air speeds noted. This will give a small range between which the real level speed must lie. The flyer must then keep the speed as steadily as possible on a reading midway between these limits, and watch the statoscope with his other eye. If it shows steady movement, one way or the other, the air speed must be altered accordingly by 1 m.p.h. In this way it is always possible at heights where the air is steady to obtain the reading correct at any rate to 1 m.p.h., even with light machines, provided always sufficient patience is exercised. The r.p.m. at this speed are then noted.

One difficulty, however, cannot be avoided. If at any height there is a steady up or down air current, then though the air may appear calm, *i.e.*, there may be no "bumps," the air speed indicator reading may be wrong, since to keep the machine *level* in an up current it is necessary to fly slightly down hill relatively to the air. Such unavoidable errors are, however, eliminated to a large extent by the method of taking speeds every 2,000 feet, and finally averaging the results.

We must now consider how the true speed of the aeroplane is deduced from the reading of the air speed indicator. It is well known that an air speed indicator reads too low at great heights—for example, if it reads 70 m.p.h. at 8,000 feet the real speed of the machine through the air is nearer 80 m.p.h. The reason for this is that the indicator, like the aneroid, is only a pressure gauge—a sensitive pressure gauge, in fact, which registers the difference of pressure between the air in a tube with its open end pointing forward along the lines of flight of the machine, and the real pressure (the static pressure) of the external air. This difference of pressure is as nearly as we can judge by experiment $= \frac{1}{2}\rho V^2$ (where $\rho$ is the density of the air and $V$ the speed of the machine), provided that the open end of the tube is well clear of wings, struts, fuselage, etc., and so is not affected by eddies and other disturbances. Now assuming this law, air speed indicators are graduated to read correctly, as I have said above, at a density of 1.221 kgm. per cubic metre, which we have taken as our standard density and called "unity." It corresponds on an average to a height of about 800 feet above sea level.

Then suppose the real air speed of an aeroplane at a height of "$h$" feet is $V$ m.p.h., and the indicated air speed is 70 m.p.h., this means that the excess pressure in the tube due to the speed is proportional to $1 \times 70^2$,

$$\text{or } \rho \times V^2 = 1 \times 70^2,$$

where $\rho$ is the density at the height in question, expressed as a fraction of the standard density. To correct the observed speed, we therefore divide the reading by the square root of the density. Thus, observation of the maximum speed of an aeroplane at a height of 8,000 feet by the locked aneroid gave 80 m.p.h. on the indicator, the temperature being 31° Fahr. From the curve we find that the density corresponding to 8,000 feet and 31° is 0.85 of standard density. The corrected airspeed is therefore:—

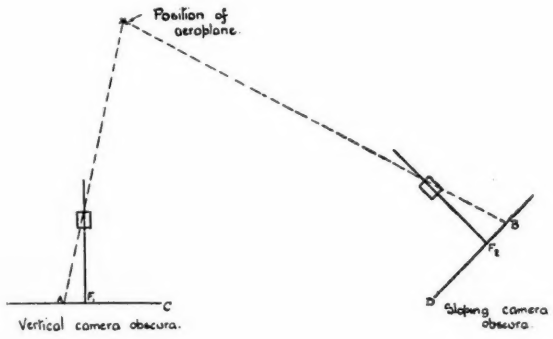
$$= \frac{80}{\sqrt{.85}} = 86.7 \text{ M.P.H.}$$

This "corrected" air speed will only be true if the above law holds, that is to say, if there are no disturbances due to the pressure head being in close proximity to struts or wings. It is always necessary to find out the magnitude of this possible error, that is, to calibrate the air speed indicator, and the only way to do this is to measure a real air speed of the aeroplane at some reasonable altitude for easy observation by actual timed observations from the ground, and from these timed results check those deduced from the air speed indicator readings. This calibration is the most important and difficult test of all, since on the accuracy

of the results depends the accuracy of all the other speed measurements. It can either be done by speed trials over a speed course close to the ground, or when the aeroplane is flying at a considerable height above the ground. In the Testing Squadron we have till lately attached more importance to the latter method, mainly because the conditions approximate more to the conditions of the ordinary air speed measurements at different heights, and because the weather conditions are much steadier and the flyer can devote more attention to flying the machine at a constant air speed than he can when very close to the ground.

One method is to use two camera obscuras, one of which points vertically upwards and the other is set up sloping towards the vertical camera. At one important testing centre the cameras are about $\frac{3}{4}$ mile apart, and the angle of the sloping camera is 45°. By this arrangement, if an aeroplane is directly over the vertical

FIGURE 3.




camera it will be seen in the field of the sloping camera if its height is anywhere between 1,000 and 16,000 feet, although at very great heights it would be too indistinct for measurements except on a very clear day. The height the tests are usually carried out is 4,000 feet to 6,000 feet.

The aeroplane is flown as nearly as possible directly over the vertical camera, and in a direction approximately at right angles to the line joining the two cameras. The pilot flies in as straight a line and at as constant an airspeed as he can. Observers in the two cameras dot in the position of the aeroplane every second. A line is drawn on the tables of each camera pointing directly towards the other camera, so that if the image of the aeroplane is seen to cross the lines in the one camera it crosses the line in the other simultaneously. From these observations it is possible to calculate the height of the aeroplane with considerable accuracy; the error can be brought down to less that 1 part in a 1,000 with care. Knowing the height, we can then calculate the speed over the ground of the aeroplane by measuring the average distance on the paper passed over per second by the image in the vertical camera. If $x$ inches is this distance, and $f$ the focal length of the lens, the ground speed is $x \times h/f$ feet per second.

It is necessary to know also the speed and direction of the wind at the height of the test. For this purpose the pilot or his observer fires a smoke puff slightly upwards when over the cameras, and the observer in the vertical camera dots in its trail every second. The height of the smoke puff is assumed to be the same as that of the aeroplane—it probably does not differ from this enough to introduce any appreciable error in the results. The true speed through the air is then found

graphically as shown in Fig. 4. Here the length AB represents the ground speed of the aeroplane as measured in the camera and CB represents on the same scale the velocity and direction of the wind. The length AC represents, also on the same scale, the true air speed of the machine.

The tests are done in any direction relative to the wind, and generally at three air speeds, four runs being made at each air speed.

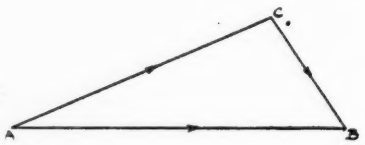
The advantages of this method are:—

(1) Being well above the earth the pilot can devote his whole attention to the test.

(2) Within reasonable limits any height can be chosen, so that it is generally possible to find a height at which the wind is steady.

(3) It does not matter if the pilot does not fly along a level path so long as he does so approximately. What is more important is that he should fly at a constant air speed.

FIGURE 4




A.B represents the measured speed of the aeroplane on the ground.
CB on the same scale represents the Velocity and direction of the wind.
Therefore AC represents the real speed of the aeroplane through the air.

(4) It is not necessary that there should be any communication between the two cameras, although it is convenient. The two tracks are made quite independently, and synchronised afterwards from the knowledge that the image must have passed over the centre line simultaneously in the two cameras.

The main disadvantage is that somewhat elaborate apparatus is necessary, but this is of not much importance in a permanent testing station.

There are often periods in war time, however, when an aeroplane has to be tested quickly, and low cloud layers and other causes prevent the camera test from being carried out. It is then necessary to rely on measurements of speeds near the ground for the calibration of the air speed indicator. In this method the aeroplane is flown about 50 feet off the ground, and is timed over a measured run. There are two observers, one at each end of the course; when the aeroplane passes the starting point the observer sends a signal and starts his stop-watch simultaneously; the second observer starts his stop-watch directly he hears the

signal, and in his turn sends a signal and stops his watch when the aeroplane passes the finishing point. By this double timing, errors due to the so-called "reaction time" of the observers are practically eliminated, for the observer at the end of the course tends to *start* his watch late, while the first observer *stops* his late. The mean of the two observations gives the real time. Four runs, two each up and down the course, are done at each air speed, the pilot or his observer noting carefully the average air speed during the run. Observations of the atmospheric pressure and temperature from which the density can be obtained are also taken. The average strength and direction of the wind during each trial are noted from a small direct reading (or recording) anemometer and the speed corrected in the same way as in the camera tests. If there is a strong cross wind the aeroplane may have to be pointed at a considerable angle to the course, and this makes the test a very difficult one to carry out well. Generally speaking, it is only reliable when the wind is quite light, not more, at any rate, than 10 m.p.h. Even this is too strong if it is a cross wind.

A further difficulty is that at high speeds, over 100 m.p.h., an aeroplane may take quite a considerable time to accelerate up to a steady speed, and so it must fly level for a long distance each end before reaching the actual course. At the testing station previously alluded to the course is a mile long, and there is a clear half mile or more at each end, but it is doubtful whether even this distance is enough for the machine to attain steady speed before the starting point. Finally, the flyer of a single-seater is generally too busy watching the ground to do more than glance at his air speed indicator more than a few times during the run. Doubtless it would be better in such a case to use some form of recording air speed instrument, although then other difficulties would arise.

Having got the true air speed from camera or speed course tests, and knowing the density at the height at which the test was carried out, we obtain what the air speed indicator should have read by multiplying the measured air speed by the square root of the density. By comparing this with the actual reading of the indicator we obtain the necessary correction. The whole procedure may be shown best by a table giving part of the results of a camera test made at the beginning of the year.

A summary of the complete speed tests may now be given. Firstly, the air speed and engine revolutions are noted flying level at full throttle every 2,000 feet approximately by aneroid. From the aneroid reading and the temperature observations at each height the density is obtained. The reading of the air speed indicator is then first corrected for instrumental errors by adding or subtracting the correction found by calibration tests over the cameras or speed course. This number is then again corrected for height by dividing by the square root of the density. The result should give the true air speed, subject, of course, to errors of observation. The numbers so obtained are plotted against the "standard" heights, *i.e.*, the average height in feet corresponding to the density during the test. A smooth curve is then drawn through the points and the air speeds at standard heights of 3,000, 6,500, 10,000, 13,000 and 16,500 read off the curve. These heights are chosen because they correspond closely with 1, 2, 3, etc., kilometres. The indicated engine revolutions are also plotted against the standard heights, because these observations form a check on the reliability of the results; also the ratio of speed to engine revolutions at different heights may give valuable information with regard to the propeller.

Table VI. gives complete results of one of our tests of air speed at heights. The table refers to the same machine as Table V., which gives the results of calibration tests of the air speed indicator. Figure 4 shows the smooth curve drawn from the calculated data, the actual air speeds calculated from the observations being shown by crosses, while the observed engine revolutions at the same heights are marked in by dots. Figure 5 gives another example, where the

## TABLE V.

Calibration Test of Air Speed Indicator No..... On.....Machine. Date 24/12/16.

| Run No. | Measured ground speed. | Measured wind speed and direction. | Corrected true (= V) airspeed. | Observed Aneroid height. | Observed temp. | Density (= $\rho$) referred to standard density. | Observed airspeed. | $V \times \sqrt{\rho}$ | Correction necessary. |
|---|---|---|---|---|---|---|---|---|---|
| 1 | 59.1 m.p.h. | 31.0 m.p.h. 161.5 | 89.2 | 5,100 | 31 | 0.879 | 80.0 | 83.6 | +3.6 |
| 2 | 123.4 ,, | 28.6 ,, 5.5 | 93.7 | 5,100 | 31 | 0.879 | 85.0 | 87.8 | +2.8 |
| 3 | 62.0 ,, | 32.3 ,, 168.5 | 93.8 | 5,050 | 31 | 0.881 | 85.0 | 88.1 | +3.1 |
| 4 | 124.7 ,, | 32.3 ,, 21.0 | 95.6 | 5,000 | 31 | 0.882 | 86.0 | 88.8 | +2.8 |

Mean +3.1

## TABLE VI.

### AIR SPEEDS AT HEIGHTS.

27/12/16.

| Aneroid height. | Temp. observed. | Standard density. | Corresponding standard height. | Observed airspeed. | Corr. for error of instrument. | Corr. for density. | Observed R.P.M. |
|---|---|---|---|---|---|---|---|
| 3,000 | 39° F. | .935 | 2,900 | 95 m.p.h. | 98 | 101½ | 1,280 |
| 5,000 | 35° | .875 | 4,900 | 93 ,, | 96 | 102½ | 1,280 |
| 7,000 | 30° | .821 | 6,900 | 88 ,, | 91 | 100½ | 1,240 |
| 9,200 | 24° | .767 | 9,000 | 81 ,, | 84 | 96 | 1,220 |
| 10,800 | 19° | .731 | 10,400 | 80 ,, | 83 | 97 | 1,220 |
| 12,800 | 17° | .682 | 12,600 | 72 ,, | 76 | 92 | 1,200 |
| 13,800 | 12° | .664 | 13,400 | 68 ,, | 72 | 88½ | 1,180 |
| 15,200 | 8° | .636 | 14,800 | 65 ,, | 69 | 86½ | 1,160 |

FINAL RESULTS FROM CURVE.

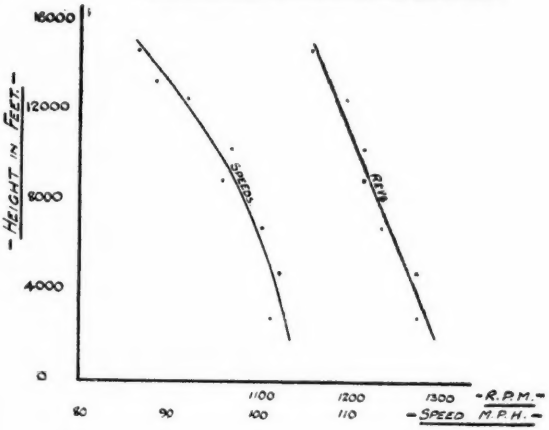
| Standard height. | Airspeed. | R.P.M. |
|---|---|---|
| 3,000 | 103.0 m.p.h. | 1,290 |
| 6,500 | 100.5 ,, | 1,250 |
| 10,000 | 96.5 ,, | 1,215 |
| 13,000 | 94.5 ,, | 1,180 |
| 15,000 | 86.0 ,, | 1,160 |

observations were very good; the air speeds and r.p.m. lie very closely on a smooth curve except at one point (about 10,000 feet) where they were probably affected by a downward current of air.

In a brief paper it is impossible to do more than explain the more important

-FIGURE 5-

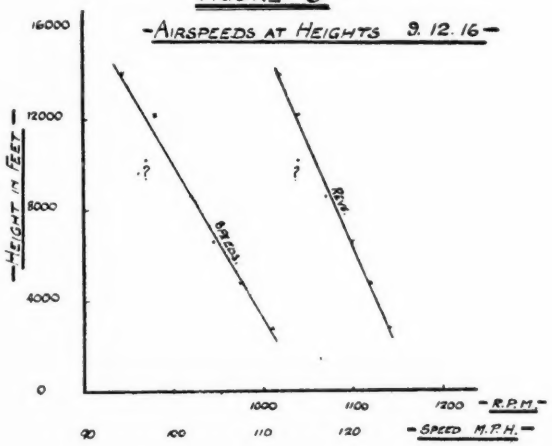
-AIRSPEEDS AT HEIGHTS 27. 12. 16-




of the "performance" tests of aeroplanes, considered solely as flying machines. For military purposes a number of tests are necessary, some of which cannot easily be reduced to figures. Nor can it be supposed for an instant that the

-FIGURE 6-

-AIRSPEEDS AT HEIGHTS 9. 12. 16-




methods outlined here are final; aeroplane testing, like all other work connected with aeroplanes, is only in its infancy; and as time goes on, and knowledge accumulates, better methods and instruments will be evolved. There are some who lay considerable emphasis on the necessity of every test instrument being

self-recording, and although this scheme appears at first sight Utopian and would relieve the pilot of a single-seater of considerable trouble, there are many objections to it when considered in detail, not the least of which is the difficulty of getting new and elaborate instruments made at a time when all manufacturers are fully engaged on other important work. When an observer can be taken I would personally place much more reliance on direct observations at the present time, and one great advantage of direct observation is that the results are *there*, and no time is lost through the failure of a recording instrument to record, a circumstance which is not unknown in practice. So far as we use recording instruments, we use them only as a check on direct observations, although we may probably adopt recording air speed indicators for the calibration tests of single seaters. But whether recording or direct reading instruments are used, it is as I said before, the flyer on whom the accuracy of the tests depends. I feel that too great stress cannot be laid on this; he is the man who does most of the experiments, and like all experimenters in every branch of science, he requires training and a great deal of practice. Although the methods themselves may be greatly changed, this much may perhaps be claimed, that the general principles on which they are founded are sound, and will only be altered in detail. The importance of the work can hardly be exaggerated; model experiments are notoriously subject to scale and other corrections, which if not carefully scrutinised may be very misleading, and it is only by accurate full scale work in addition that we can hope to maintain a steady improvement in the efficiency of aeroplanes.

# THE SCREW PROPELLER IN AIR.

BY M. A. S. RIACH, A.F.AË.S.

## INTRODUCTORY.

At the present time there does not appear to be any really adequate theory of the airscrew. The only theory that has been developed recently is the one given by Mr. F. W. Lanchester in a paper to the Institution of Automobile Engineers in April, 1915.* This paper is, moreover, admittedly based on certain theoretical conceptions, and not directly upon aerofoil data obtained from wind channel tests.

I have always believed in the possibility of successfully applying aerofoil data to propellers, and any method that attempted to co-ordinate the results of wind channel experiments on model aeroplane wings and on model airscrews appeared to me to be worthy of every encouragement.

It was for these reasons that the so-called blade element method of propeller analysis, in the first instance enunciated by S. Drzewiecki and later developed by Mr. F. W. Lanchester in his work "Aerial Flight," appeared to me to be a step in the desired direction, and the few experiments carried out at the National Physical Laboratory to test the accuracy of the theory, and as published in the Report of 1912-13, led me to believe that the method might with advantage be developed analytically so as to form a reasonably sound basis for the comparison of experimental results and to give to the designer a fairly clear outlook upon the subject.

It has always appeared to me that, when testing any theory, however empirical, a distinct advantage is obtained by first stating the whole of the premises, and, having got these clearly defined, to work out the results of the initial assumptions to their extreme logical conclusions. However approximate a theory may eventually turn out to be, it saves time in the long run to fully develop the theoretical aspect of the subject which is often capable of outlining new methods of attacking the problem even when the original conceptions upon which the theory was based have been shown to possess no longer a sufficient degree of exactitude.

As an example of this, I might quote the great importance of pitch-ratio upon efficiency in the design of airscrews, and it is difficult to see how such a conclusion could have been arrived at without the help of such analysis as that proposed.

As a consequence I was led to investigate the subject from the point of view of the blade element theory, and the results so obtained are given in my book "Airscrews."†

With the information now available upon the subject it has been found that this method is not sufficiently accurate even for the comparatively "rough and ready" methods of commercial propeller design, where the employment of

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* "The Screw Propeller," by F. W. Lanchester. April 1915, I.A.E.

† "Airscrews," by M. A. S. Riach. (London: Crosby, Lockwood and Son).

empirical correction factors has been found to be necessary, and although it will probably be found that the theory is still quite a good guide and, if employed in conjunction with the correct correction factors, may usually be trusted to give quite sufficiently accurate results for the ordinary "hack" propellers demanded of the designer in commercial design work, yet, viewed from the much more rigid standpoint of the degree of accuracy obtained in a laboratory experiment, it must be confessed that the method still leaves much to be desired.

A number here to-night will probably remember Mr. A. R. Low's paper on "Airscrews" read before the Society in April, 1913. In the discussion which followed the reading of this paper, Mr. Handley Page pointed out that Mr. Low had dealt with the propeller problem from the point of view of what happens to the air as it passes across the blade, considered on the assumption that the blade elements were entering undisturbed and non-accelerated air, and that in reality it did not by any means follow that the lift coefficients and lift-drag ratios taken for the section as tested as an aerofoil in a wind channel would be identical with the results obtained when the section formed part of an airscrew blade revolving in a helical path and encountering disturbed air, and that in consequence the coefficients would require modification to accord with practical results obtained.

This, Mr. Page went on to say, really constituted the second half of the problem, and dealt with the question from the point of view of the slip stream on the basis of the Rankine or Froude theory.

He expressed the opinion that there should be no antagonism between the two methods of design, but that they should rather be used in conjunction for the correct determination of all the constants of the propeller blade.

It was with a view of co-ordinating these two methods of attacking the problem that led me some time ago to consider whether an advance might not be obtained by utilising both of these theories in an attempt to take into account certain factors ignored in both methods considered separately, and by which the determination of some of the empirical constants used in practical design could be brought about, with the result of more closely co-ordinating theory and experiment.

Before entering upon a detailed discussion of the propeller theory given in this paper, I propose to commence with a formal presentation of the problem, as conceived by the majority of writers upon the subject, notably Rankine and R. E. Froude, and as given in standard text books on the screw propeller of marine engineering practice.

The subject is by no means a non-controversial one, the theory advanced by Mr. R. E. Froude in his papers read before the Institution of Naval Architects in 1889 and 1911* having been violently attacked by Professor Henderson in the discussion of these two papers and in his own paper of 1910.†

## GENERAL ANALYSIS.

Let:—

$V$ = the speed of advance of the screw relative to the undisturbed fluid.
$V + V_1$ = the speed of the fluid at the actuator disc relative to the screw.
$V + V_1 + V_2$ = the speed of the fluid after passing through the actuator relative to the screw.
$M$ = the mass per second of the fluid through the actuator.

Then, according to R. E. Froude, the thrust on the screw is equal to

$$M \,.\, (V_1 + V_2),$$

and the useful work done per second is equal to

$$M \,.\, (V_1 + V_2) \,.\, V$$

* Trans. I.N.A., Vol. XXX. Vol. LIII. † I.N.A., Vol. LII.

and the total work done per second is equal to

$$M \cdot (V_1 + V_2) \cdot (V + V_1)$$

and therefore the efficiency is given by

$$\frac{V}{V + V_1}, \text{ and also } V_2/V_1 = 1,$$

*i.e.*, one half the acceleration takes place in front, and one half behind the actuator.

But, according to Professor Henderson, the acceleration ahead of the actuator does not contribute to the thrust of the screw, but forms part of a conservative system and that in consequence the thrust, work, and efficiency are given by :—

$$\text{Thrust} = M.V_2$$
$$\text{Useful work/sec.} = M.V_2.V$$
$$\text{Total work/sec.} = \tfrac{1}{2}.M \cdot [(V + V_1 + V_2)^2 - (V + V_1)^2]$$

and therefore efficiency

$$= \frac{V}{V + V_1 + V_2/2}$$

Owing to the fact, however, that we are unable to determine the values of $V_1$ and $V_2$ for each radius along the blade, the foregoing theories are insufficient for the practical design of airscrews, and in consequence one is forced to experiment and its corresponding empirical methods.

## THE BLADE ELEMENT THEORY.

The most generally accepted theory of the airscrew at the present time is the one in which the reactions on the blades are calculated from the reactions on elementary strips of the blade, determined on the basis of experimental work carried out in wind channels on aerofoil sections.

In this method certain empirical " correction factors " have been found necessary in order to bring calculated results into agreement with those obtained by experiment.

It is usual to make allowance for these correction factors either at the commencement or at the end of the analysis in the practical design of airscrew blades.

The method of analysis is briefly as follows :—

Let Fig. 1 represent a section in plan and elevation of an airscrew blade cut off by two concentric radii of $(x)$ and $(x + dx)$ respectively.

Let the angle made by the chord of the section with the direction of rotation, or disc of revolution be denoted by $(\phi)$.

Let the airscrew have an axial velocity of $(V)$ in the direction of $OY$, and let it have a revolution speed of $(n)$, so that the axial advance per revolution is $(V/n)$.

Let the angle of the effective helix be $(A)$, and denote the angle $(\phi - A)$ by $(a)$, which may then be called the " apparent angle of attack."

It is obvious from the figure that $(a)$ is only the angle between the chord line and the relative wind so long as there is no acceleration of air going on in front of the propeller disc, for otherwise the true " angle of attack " of the section under consideration is less than $(a)$, due to the inflowing velocity ahead of the screw. It is the variations in the analysis caused by an inflowing axial velocity which make it necessary to employ correction factors, although were it possible

to predict the amount of such an inflow, the necessity for such empiricism would probably largely cease to exist. The necessary modification in the analysis to take account of the added axial velocity ahead of the actuator is quite a simple matter, and the trouble is that the amount of such axial velocity is not known.

We will therefore start by investigating the geometrical relations which exist between the various component forces assumed to be acting upon an element of an airscrew blade, and for this purpose we will introduce a modification into the preceding statement of the general analysis by considering the effect of the addition of an axial velocity $(V_1)$ ahead of the screw disc. Figure 1 will now become modified as in Figure 2, and the geometrical statement of the problem is then as follows:—

Let Fig. 2 represent a section in plan and elevation of an airscrew blade cut by two concentric radii of $(x)$ and $(x+dx)$ respectively.

Let the angle made by the chord of the section with the direction of rotation, or disc of revolution, be denoted by $(\phi)$.

Let the airscrew have an axial velocity of $(V)$ and a revolution speed of $(n)$, so that the axial advance per revolution is $(V/n)$.

Let there be an added axial velocity of inflow $(V_1)$ immediately ahead of the actuator disc.

Let the angle of the effective helix be $(A)$ and denote as before the angle $(\phi - A)$ by $(a)$ so that $(a)$ represents the "angle of attack" of the section when $(V_1)$ is equal to zero. But generally when $(V_1)$ is not zero the real "angle of attack" is not $(a)$ but $(a_1)$ as in the figure, and then, for all positive values of $(V_1)$, $(a_1)$ is less than $(a)$, and we may then denote the angle between the disc of revolution and the relative wind by $(A_1)$.

We then have at once the obvious relations:—

$$\phi = A + a = A_1 + a_1$$
$$\text{and } \therefore\ A_1 = A + a - a_1 = \phi - a_1$$

and let $(b)$ denote the width of the section at radius $(x)$.

Then we are in a position to at once write down the forces acting upon the element from a consideration of the geometry of the figure.

Let Fig. 3 represent the forces acting upon the section at radius $(x)$.

$(L_1)$ denotes the lift of the section considered as an aerofoil.
$(D_1)$ ,, drag ,, ,, ,, ,,

Then the resolved parts of $(L_1)$ and $(D_1)$ in an axial direction are:—

$$L_1 \cdot \cos A_1 - D_1 \cdot \sin A_1$$

and the resolved parts of these two forces in a transverse direction parallel to the disc of revolution are

$$L_1 \cdot \sin A_1 + D_1 \cdot \cos A_1$$

The first component represents the thrust of the blade element, and the second component represents the torque of the element divided by the radius.

We then have the two following equations for the thrust and torque of the blade element.

Writing $(dT_1)$ for the thrust, and $(dM_1)$ for the torque we have

$$dT_1 = L_1 \cdot \cos A_1 - D_1 \cdot \sin A_1$$
$$dM_1 = x \cdot (L_1 \cdot \sin A_1 + D_1 \cdot \cos A_1)$$

and the efficiency of the element is given by

$$\eta = \frac{dT_1 \,.\, V}{dM_1 \,.\, 2.\pi.n} = \frac{\tan A}{\tan (A_1 + \gamma_1)}$$

where

$$\tan \gamma_1 = \frac{D_1}{L_1}$$

Now returning to the thrust and torque equations we see that since $(L_1)$ denotes the elementary lift on the blade at a radius of $(x)$, we may write

$$L_1 = cy_1.\rho.b.dx.v^2$$
$$= cy_1.\rho.b.dx \,.\, (2.\pi.n.x)^2 \,.\, \sec^2 A_1$$
$$\text{since } v = 2.\pi.n.x. \sec A_1$$

Where

$(cy_1)$ denotes the absolute lift co-efficient of the section at radius $(x)$.
$\rho$ denotes the mass/density of the fluid.
$(b)$ denotes the blade width of the section at radius $(x)$.

So that for the thrust of the element we have

$$dT_1 = cy_1.\rho.b.dx.4.\pi^2.n^2.x^2. \sec A_1. [1 - \tan A_1. \tan \gamma_1]$$

So that, denoting by $(r_0)$ and $(r)$ the inside and outside blade limits, usually taken to be the boss and tip, we get by integration:—

$$T_1 = 4.\pi^2.n^2.\rho. \int_{r_0}^{r} cy_1.b.x^2. \sec A_1. [1 - \tan A_1. \tan \gamma_1] \,.\, dx$$

giving the total thrust on each blade of the airscrew under the conditions imposed by an axial velocity of inflow.

And similarly for the total torque on each blade of an airscrew we have

$$dM_1 = \rho.b.cy_1.dx.x^3.4.\pi^2.n^2. \sec A_1. (\tan A_1 + \tan \gamma_1)$$

and therefore the total torque on each blade is given by

$$M_1 = 4.\pi^2.n^2.\rho. \int_{r_0}^{r} cy_1.b.x^3. \sec A_1. (\tan A_1 + \tan \gamma_1) \,.\, dx$$

and hence the brake horse-power $(H)$ necessary to turn the airscrew is given by, denoting by $(N)$ the number of blades,

$$H = \frac{N.8.\pi^3.n^3.\rho. \int_{r_0}^{r} cy_1.b.x^3. \sec A_1. (\tan A_1 + \tan \gamma_1) \,.\, dx}{550}$$

in lb., foot, second, units, and where $(\rho)$ has the value of (.00238).

The total efficiency of the whole blade is obtained as follows from the ratio-total useful work done per second to total work put in per second.

$$\eta_1 = \frac{V. \int_{r_0}^{r} cy_1.b.x^2. \sec A_1. (1 - \tan A_1. \tan \gamma_1) \,.\, dx}{2.\pi.n. \int_{r_0}^{r} cy_1.b.x^3. \sec A_1. (\tan A_1 + \tan \gamma_1) \,.\, dx}$$

Now none of these formulæ can be applied unless the value of $(V_1)$ for every radius $(x)$ is known, so that $(A_1)$ and its functions are also known.

This brief summary of the geometrical analysis forms what may be termed the blade element aerofoil portion of the theory as enunciated in this paper and is by way of being a mathematical tool for the investigation of problems of this nature provided that the required values of the inflow velocity at each radius are known.

## R. E. FROUDE'S THEORY.

I now propose to deal with the R. E. Froude hypothesis of screw propulsion in fluids and its application to the present theory of the airscrew.

Using the same symbols as those already defined, we commence by considering the thrust and efficiency of an annulus at a radius of $(x)$ from the boss centre. The thrust contributed by the annulus is due to the change in momentum per second of the fluid passing through the actuator disc at the radius under consideration.

In the paper contributed by Mr. R. E. Froude to the Institution of Naval Architects in 1889* it is shown that the thrust of such a propulsive system as that contemplated in the action of the screw propeller is equal to the product of the mass per second of fluid passing through the actuator and the difference in velocity between the slip or wake stream and the translational velocity of the screw through the fluid.

That is the thrust on an annulus is given by

$$T = M \,.\, (V_1 + V_2) = M.V_1 \,.\, \left(1 + \frac{V_2}{V_1}\right)$$

It may be shown that, on R. E. Froude's assumption of the total work done per second being equal to the gain in kinetic energy of the slip stream, the added slip velocity $(V_2)$ is equal to the inflow velocity $(V_1)$, *i.e.*, $(V_2/V_1)$ is equal to unity.

For the present, however, I prefer to keep this ratio in its algebraical form and if necessary to substitute for it values other than unity as occasion may determine.

Now the area of the annular ring element at radius $(x)$ is $(2.\pi.x.dx)$, and consequently, if the whole of this area be assumed to be operative in producing the thrust, we can write:—

$$\text{Mass per second through annulus} = \rho.2\pi x.dx \,.\, (V + V_1)$$

and this must be for continuity of motion the value of the mass of fluid passing any given point in the streams ahead and behind the screw per second.

Hence, if we introduce a co-efficient $(q)$ to represent the fraction of the annular ring element employed, we have

$$M = q.\rho.2.\pi.x.dx.\,(V + V_1)$$

and therefore the thrust on the annulus becomes

$$T = q.\rho.2.\pi.x.dx.\,(V + V_1)\,.\,V_1.\left(1 + \frac{V_2}{V_1}\right)$$

and if $(q)$ is equal to unity, then the whole ring area is assumed to be operative, and if the ratio $(V_2/V_1) = 1$, on R. E. Froude's theory, we obtain:—

$$T = \rho.4.\pi.x.dx.\,(V + V_1)\,.\,V_1$$

and the only unknown quantity is then $(V_1)$.

* Trans. I.N.A., Vol. XXX.

If $(V_1)$ remained constant over the whole screw disc, we could integrate the above equation and obtain the total thrust on the screw from the formula :—

$$T = \frac{\rho.\pi.d^2.V_1.(V+V_1)}{2}$$

so that if

$$\rho = .00238$$
$$V = 100$$
$$V_1 = 20$$
$$d = 8$$

we get the total thrust equal to 540lbs. On the other hand, if $(V_2/V_1)=0$, an extreme condition contemplated by Mr. F. W. Lanchester,* the thrust of the screw is halved, *i.e.*, 270lbs.

Again the efficiency of the annulus is obtained from

$$\eta = \frac{\text{thrust} \times V}{\text{thrust} \times (V+V_1)} = \frac{V}{V+V_1}$$

and this is on the assumption of a frictionless fluid. This efficeincy formula may also be written in the form :—

$$\eta = \frac{\tan A}{\tan A_1}$$

where $(A)$ and $(A_1)$ have the values already defined.

Now comparing this formula of R. E. Froude with that already obtained from the blade element theory, viz. :—

$$\eta = \frac{\tan A}{\tan (A_1+\gamma_1)}$$

we notice that the two formulæ become identical when $(\gamma_1)$ is equal to zero, *i.e.*, when there is no drag as in the R. E. Froude hypothesis of a frictionless fluid.

## THE TWO METHODS COMBINED.

Having thus briefly enunciated the two methods of attacking the problem, I propose to combine them by equating the two expressions for the thrust on an element obtained from each.

This will then give the lift co-efficient $(cy_1)$ of the section at $(x)$ in terms of $(V_1)$ the inflow velocity, that is in terms of $(a_1)$ the real angle of attack.

A graph of the lift co-efficient may then be plotted against the angle of attack, and by superimposing the graph obtained from the wind channel for the section considered as an aerofoil, we obtain the true values of the lift co-efficient and inflow velocity at the radius under consideration. This is evident when it is considered that the lift co-efficient is a function of the angle of attack or angle of incidence, and that in consequence were the form of this function known analytically we might substitute for it in the formula obtained by equating the thrusts and thus solve for both of these variables at once.

The graphical method is employed owing to the fact that the analytical form of functions of this kind is usually difficult to determine accurately.

* "A Contribution to the Theory of Propulsion and the Screw Propeller," by F. W. Lanchester. Trans. I.N.A., 1915.

The method of procedure is as follows:—

The geometrical blade element aerofoil expression for the thrust is

$$N.dT_1 = N.cy_1.\rho.b.dx.4.\pi^2.n^2.x^2.\sec A_1.[1 - \tan A_1.\tan \gamma_1]$$

where $(N)$ denotes the number of blades employed, and the momentum change expression on R. E. Froude's theory is

$$T = q.\rho.2.\pi.x.dx.V_1.(V + V_1).\left(1 + \frac{V_2}{V_1}\right)$$

$$= \left(1 + \frac{V_2}{V_1}\right).q.\rho.2.\pi.x.dx.(2.\pi.n.x)^2.\tan A_1.[\tan A_1 - \tan A]$$

in terms of $(A)$ and $(A_1)$. So that equating the two expressions we obtain

$$cy_1 = \frac{q.2.\pi.x.\left(1 + \frac{V_2}{V_1}\right).\sin A_1.[\tan A_1 - \tan A]}{N.b.[1 - \tan A_1.\tan \gamma_1]}$$

which is the expression sought and which when employed in conjunction with the wind channel graph of $(cy_1)$ against $(a_1)$ is sufficient to solve for both of these variables.

The two empirical unknown factors in the above equation are $(q)$ and $(V_2/V_1)$.

## EXPERIMENTAL VERIFICATION.

We may, I think, for a first estimate take both $(q)$ and $(V_2/V_1)$ as being equal to unity. That is, we assume that the full ring area of the annulus is utilised, and that R. E. Froude's law holds with regard to the relative values of $(V_1)$ and $(V_2)$, in other words that one half the acceleration is precedent and one half antecedent to the propeller.

Now consider for a moment the effect on the formula just established of keeping the quantity $(V/n.d)$, the pitch ratio, a constant.

It is immediately apparent that if $(V/n.d)$ remains constant then for any given radius $(x)$ the following remain constant also—$(q)$, $(V_2/V_1)$, $(b)$, $(A)$, $(\phi)$, and also the following are constant—$(d)$, $(N)$.

So that the curve plotted of $(cy_1)$ against $(a_1)$ will also remain constant, as will also the wind channel curve superimposed on this curve, since the section is constant. Hence, the points of intersection of these two curves will also remain constant, that is that $(A_1)$ is constant, and therefore

$$\frac{\tan A_1}{\tan A} = \frac{V + V_1}{V}$$

is constant also.

We thus arrive at the result that, for a given value of $(V/nd)$, the ratio of the velocity of the fluid through the actuator disc to the velocity of the undisturbed fluid remains a constant. This result has been established experimentally by G. Eiffel in his tests on model airscrews.*

We thus have in the above example a direct comparison between the results given by the theory, and those obtained by experiment, and it is gratifying to know that in this case experimental results fully confirm theory.

* "Nouvelles Recherches sur la Résistance de l'Air et l'Aviation," 1914.

It is especially interesting to analyse an existing propeller and to find the changes in thrust, work, and efficiency which are obtained by alteration of one or more of the propeller characteristics.

Let Fig. 4 denote the two graphs of $(cy_1)$ against $(a_1)$ for a given radius $(x)$ for some particular type of propeller.

Then the expressions for thrust and efficiency on the element are:—

$$dT_1 = cy_1.\rho.b.dx.4.\pi^2.n^2.x^2.\sec A_1.[1 - \tan A_1.\tan \gamma_1]$$

and

$$\eta = \frac{\tan A}{\tan (A_1 + \gamma_1)}$$

Now examine the graph given in Fig. 4, and suppose the section of the blade to remain constant while the blade width is increased.

The chord angle $(\phi)$ also remaining constant throughout, so that variation in $(A_1)$ is obtained by variation in $(a_1)$.

Then if we double the blade width, we halve the ordinates of curve (1) and since curve (2) remains constant throughout the resulting values of $(cy_1)$ and $(a_1)$ are smaller than before. This increases $(A_1)$ and probably also $(\tan \gamma_1)$, but decreases $(cy_1)$, so that the new thrust value may be obtained in this manner for any given case. It will usually be found that, since $(\sec A_1)$ has a value of nearly unity, the new value of the thrust will not be anything like double the old value, due to doubling the blade width. The percentage increase in fact decreases with increase in blade width, and this result is, I believe, confirmed by experiments on marine screws.

The reason for this loss of thrust I contend is due to the higher inflow velocity $(V_1)$ and consequently smaller angle of attack $(a_1)$. It may, it is true, be argued that if $(V_1)$ be increased, then the thrust must correspondingly increase, although not proportionally, and in fact this is a necessary condition providing that the ratio $(V_2/V_1)$ remains constant throughout.

I shall endeavour to show, however, that the ratio $(V_2/V_1)$ is by no means to be regarded as a remaining constant for changes in angle of attack $(a_1)$, and that for small or negative angles of attack this ratio may conceivably become zero or even negative, depending upon the pressure distribution round the blade section at the angle of attack $(a_1)$ considered.

Turning now to the efficiency of the element denoted by

$$\frac{\tan A}{\tan (A_1 + \gamma_1)}$$

it is evident that as the blade width is increased, both $(A_1)$ and $(\gamma_1)$ are increased, thus reducing the efficiency—a result again in accordance with experience of marine screws.

On the other hand it is evident that the efficiency with wide blades may be considerably improved by increasing the chord angle of the blade $(\phi)$ so as to give to the angle of attack of the section a value corresponding to a minimum value of $(\gamma_1)$.

In this case, therefore, the efficiency of the element is increased by increasing the so-called slip angle of the screw. This result is again in accordance with experiment on marine propellers.

## EXTENSION OF THE THEORY.

We may, I think, roughly divide a propeller blade into three parts, the boss, the tip, and the centre forming the main portion of the blade. It is usual, I

believe, when designing propellers on the old aerofoil theory to choose for the sections an average aspect ratio of say six, and then to employ a correction factor obtained by experiment in determining the blade widths along the blade. This process, however, seems to be open to objection from various sources. It is well known that the pressure distribution along the span of any aerofoil, such as an aeroplane wing for instance, does not remain constant but varies from a maximum intensity in the middle of the span to a minimum at the wing tips, and it is this fact of tip losses which makes the high aspect ratio aerofoil more economical than one of lower aspect ratio.

The same consideration should apply to the blades of an airscrew, where each individual blade can be treated as a separate aerofoil, and where in consequence the end losses—in this case those of the boss and tip—must be taken into account in estimating the thrust, work, and efficiency.

The boss conditions in this case may probably be ignored as being very small; the tip losses form, however, a fundamental link in the chain of analysis and in consequence require most careful consideration.

The exact analogy between the pressure distribution along the span of an aerofoil considered as an aeroplane wing, and considered as a blade of a screw propeller, does not appear to be immediately capable of demonstration. As a first estimate, however, I have assumed the maximum intensity of pressure to occur at about the centre of the blade, *i.e.*, at a radius of $\frac{1}{4}$ diameter, and to fall off to a minimum value at the boss and tip—that is I consider the propeller blade as the equivalent of an aeroplane wing of infinite span revolving about one of its wing tips.

Under these assumptions then it is possible to form some idea of the relative changes in lift and lift/drag which occur as we proceed along the blade from the boss to the tip.

So that if, when analysing any existing airscrew, we apply the theory of inflow already enunciated under the conditions imposed by the pressure distribution change considered above, we shall, I think, be in a position to form a fairly correct picture of the actual conditions under which the blades are working and hence be in a position to estimate the probable values of the thrust, work, and efficiency, with a much larger measure of success than by the older method in which such refinements as those outlined here are ignored.

There is still the question of the values of the ratio $(V_2/V_1)$ to be allotted to the various sections along the blade of the airscrew, as it may be taken for granted, I think, that this ratio not only does not remain constant over the entire surface of the screw disc, but that its numerical value may usually be taken as considerably less than unity, and sometimes even zero or negative.

That values other than unity have been considered not only possible, but probable for this ratio by authorities on the marine screw propeller, may be gauged from the following remarks by D. W. Taylor on the discussion of Mr. R. E. Froude's 1911 paper.*

Mr. D. W. Taylor said:—"It is true that the experimental curves appear to indicate that the acceleration takes place mostly forward of the screw, and is not equally divided between forward and aft as deduced by Mr. Froude for his actuator. . . . This indication that with actual propellers the greater portion of the fore and aft acceleration of the water takes place forward of the screw," etc.

It appears to me in this connection not unreasonable to regard this $(V_2/V_1)$ ratio as equal to the ratio of the lower and top pressures on an aerofoil section, and if this be applied to all radii along the blade we are immediately in a position to analyse any given airscrew blade under any set of conditions. For instance,

* Trans. I.N.A., Vol. LIII.

to take as an example the case of the section under the conditions when giving no thrust.

Then

$$dT_1 = L_1 \cdot \cos A_1 - D_1 \cdot \sin A_1 = 0$$

$$\text{and } \therefore \frac{D_1}{L_1} = \tan \gamma_1 = \cot A_1$$

and if $(\phi)$ is known, then we can solve for $(a_1)$ since $(\gamma_1)$ is known from the wind channel curve.

But if

$$dT_1 = 0, \text{ then } V_1 \cdot \left(1 + \frac{V_2}{V_1}\right) = 0,$$

and hence either $(V_1)$ is equal to zero or $(V_2/V_1)$ is equal to minus unity. In the first case, there is no inflow, and in the second case the top pressure is numerically equal to the under surface pressure, but acts in an opposite direction, and in this case the angle of attack $(a_1)$ of the section would be probably negative.

Now tests on aerofoils show that the pressure distribution at about $(-2^\circ)$ is about the same numerically on top and bottom surfaces, but that the bottom surface pressures act in an opposite direction to the usual at positive angles of incidence.

These facts appear to bear out the statement that $(V_2/V_1)$ may have a negative value.

## SPECIAL CASES.

I now propose to give a solution for the total efficiency of an airscrew having a certain type of blade outline under the hypothesis of inflow, and also a formula for the B.H.P. necessary to sustain a given weight without axial advance through the air. This latter constitutes the problem of the helicopter.

For simplicity in the analysis in both the above cases, a form of blade outline analogous to that defined by Mr. A. R. Low as the "Rational Form" will be employed. The difference between Mr. Low's blade function and the one here adopted lies in the modification introduced as the result of the conception of an inflowing velocity ahead of the screw disc altering the value of the "gap" or distance between consecutive blade paths.

Fig. 5 explains this point.

The "gap" is seen to have a value of

$$\frac{2.\pi.x.\sin A_1}{N}$$

where $(N)$ denotes the number of blades of the airscrew.

Hence, making the blade function $f(x)$ equal to the "gap" we have:—

$$f(x) = \frac{2.\pi.x.\sin A_1}{N}$$

$$\text{and } \therefore b = c_1.f(x) = \frac{c_1.2.\pi.x.\sin A_1}{N}$$

Now substitute this value for the blade width ($b$) in the formula obtained from equating the thrusts, and we get:—

$$\tan A_1 = \frac{2.\pi.x.c_1cy_1 + f.P}{2.\pi.x.\ (f + c_1.cy_1.\ \tan \gamma_1)}$$

where ($P$) denotes the effective pitch of the airscrew, *i.e.*, the advance per revolution, and $f = q.\ (1 + V_2/V_1)$. And the value of ($b$) in terms of ($c_1$) is obtained from the formula:—

$$b = \frac{c_1.2.\pi.x}{N} \cdot \frac{2.\pi.x.c_1.cy_1 + f.P}{\sqrt{4.\pi^2.x^2.\ [f + c_1.cy_1.\ \tan \gamma_1]^2 + [2.\pi.x.c_1.cy_1 + f.P]^2}}$$

and ($c_1$) is the value in this case of

$$\frac{\text{Blade width at radius } (x)}{\text{Gap at radius } (x)}$$

Very roughly this formula for ($b$) boils down to:—

$$b = \frac{c_1.P}{N} \text{ and } \therefore\ c_1 = \frac{b.N}{P}$$

So that if ($b$) = 1ft., ($P$) = 6ft., ($N$) = 2 blades, we have approximately:— ($c_1$) is equal to $\frac{1}{3}$, *i.e.*, the gap/chord ratio of the blade is 3 : 1 at this radius.

Now return to the general efficiency formula for any type of airscrew blade, viz.:—

$$\eta_1 = \frac{P}{2.\pi} \cdot \frac{\int_{r_o}^{r} cy_1.b.x^2.\ \sec A_1.(1 - \tan A_1\ .\ \tan \gamma_1)\ .\ dx}{\int_{r_o}^{r} cy_1.b.x^3.\ \sec A_1.(\tan A_1 + \tan \gamma_1)\ .\ dx}$$

and substitutes for ($A_1$) and its functions from the formula already established for (tan $A_1$).

I am, for simplicity and the sake of comparison, taking the lift co-efficients and lift/drag ratios as remaining constant over the whole blade, and the value of ($r_o$) as zero.

This approximation does not invalidate the value of the results so obtained from the point of view of a guide to the suitable choice of the other parameters involved, such as blade width and pitch ratio.

After integration and simplifying we obtain for the efficiency of an airscrew having a "Rational" form of blade outline the following formula:—

$$\eta_1 = \frac{5.Z}{\pi} \cdot \frac{3.c_1.cy_1.\pi^2 + 4.\pi.f.Z.\ (f - c_1.cy_1.\ \tan \gamma_1) - 6.f^2.\ \tan \gamma_1.Z^2}{12.c_1.cy_1.\pi^2.\ (f.\ \tan \gamma_1 + c_1.cy_1.\ \sec^2 \gamma_1) + 15.\pi.f.Z.\ (c_1.cy_1. + c_1.cy_1.\ \sec^2 \gamma_1 + f.\ \tan \gamma_1) + 20.f^2.Z^3}$$

where $Z = \frac{V}{n.d}$

It is interesting to notice that the formula contains both $(c_1)$ and $(cy_1)$, *i.e.*, the efficiency is made to depend both upon lift co-efficient and blade width, since $(c_1)$ is a function of blade width.

This is as might be expected from the initial hypotheses contained in the theory of inflow developed here.

A graph of efficiency against pitch ratio $(Z)$ is given in Fig. 6 from this formula for various values of $(c_1)$. It will be noticed that as $(c_1)$ increases, *i.e.*, as $(b)$ increases, the total blade efficiency decreases.

In a recent paper on the subject,* Mr. F. W. Lanchester gave the following formula for the B.H.P. necessary to sustain a given weight in the air. This formula was based directly upon the theory advanced by Mr. R. E. Froude.

Mr. Lanchester's formula was

$$\frac{W}{H} = 550 \cdot \sqrt{\frac{2.\rho.A}{W}}$$

This assumes the inflow velocity to remain constant over the blade as well as the ratio $(V_2/V_1)$ which has the value of unity given to it by R. E. Froude.

Consider the general efficiency formula, viz. :—

$$\eta_1 = \frac{P}{2.\pi} \cdot \frac{\int_{r_0}^{r} cy_1.x^2.f(x).\sec A_1.(1 - \tan A_1.\tan \gamma_1).dx}{\int_{r_0}^{r} cy_1.x^3.f(x).\sec A_1.(\tan A_1 + \tan \gamma_1).dx}$$

in conjunction with that already established, viz. :—

$$cy_1 = \frac{q.2.\pi.x.\left(H\frac{V_2}{V_1}\right).\sin A_1.[\tan A_1 - \tan A]}{N.b.(1 - \tan A_1.\tan \gamma_1)}$$

and therefore putting $(b) = c_1.f(x)$, we get

$$f(x) = \frac{q.2.\pi.x.\left(H\frac{V_2}{V_1}\right).\sin A_1.[\tan A_1 - \tan A]}{cy_1.N.c_1.(1 - \tan A_1.\tan \gamma_1)}$$

whence substituting for this function $f(x)$ in the general efficiency formula, we get :—

$$\eta_1 = \frac{P}{2.\pi} \cdot \frac{\int_{r_0}^{r} x^3.\left(1 + \frac{V_2}{V_1}\right).\tan A_1.(\tan A_1 - \tan A).dx}{\int_{r_0}^{r} \frac{x^4.\tan A_1.\left(1 + \frac{V_2}{V_1}\right).(\tan A_1 - \tan A)(\tan A_1 + \tan \gamma_1).dx}{1 - \tan A_1.\tan \gamma_1}}$$

and now if we make $(V_1)$ and $(V_2/V_1)$ constant over the blade, which Mr. Lan-

* "A Contribution to the Theory of Propulsion and the Screw Propeller," by F. W. Lanchester. Trans. I.N.A., 1915.

chester defines as the condition for maximum efficiency, and $(r_0)$ equals zero, we get :—

$$\eta_1 = \frac{P}{2.\pi} \cdot \frac{\int_{r_0}^{r} x.dx}{\int_{r_0}^{r} \frac{x^2.\,(V+V_1+2.\pi.n.x.\,\tan\gamma_1)\,.\,dx}{2.\pi.n.x - \tan\gamma_1.\,(V+V_1)}}$$

and if $\tan\gamma_1 = 0$, then $\eta_1 = V/V+V_1$, thus reducing to the ordinary R. E. Froude formula.

But in practice $\tan\gamma_1$ is always greater than zero, and therefore evaluating the above, we get :—

$$\eta_1 = \frac{3.V.\pi^2.n^2}{8} \cdot \frac{d^2.\,(1-h^2)}{\frac{\tan\gamma_1}{4} \cdot [\,\{\pi.n.d - \tan\gamma_1.\,(V+V_1)\}^3 - \{\pi.n.d.h - \tan\gamma_1.\,(V+V_1)\}^3] + \frac{3.\,(V+V_1)}{8} \cdot [1+3.\tan^2\gamma_1] \cdot [\,\{\pi.n.d - \tan\gamma_1.\,(V+V_1)\}^2 - \{\pi.n.d.h - \tan\gamma_1.\,(V+V_1)\}^2] + \frac{3.\tan\gamma_1.\,(V+V_1)^2}{2} \cdot \left[1 + \frac{3.\tan^2\gamma_1}{2}\right] \cdot \pi.n.d\,[1-h] + \frac{3.\tan^2\gamma_1.\,(V+V_1)^3}{4} \cdot \sec^2\gamma_1.\log_e\left[\frac{\pi.n.d - \tan\gamma_1.\,(V+V_1)}{\pi.n.d.h - \tan\gamma_1.\,(V+V_1)}\right]}$$

Where $h = r_0/r$, a fraction which is greater than zero in this case of discontinuous blade outline.

Now consider the B.H.P. necessary to sustain a total weight of $(W)$ lbs. in the air.

The B.H.P. required at an axial speed upwards of $(V)$ ft. per second is given by

$$H = \frac{W.V}{550.\eta_1}$$

and therefore substituting for $(\eta_1)$ the value given from Mr. Low's "Rational" blade form we get, making $(V)$ equal to zero,

$$\frac{W}{H} = \frac{550.5.d.cy_1.c_1\sqrt{\pi.\rho.f}}{8\sqrt{2.W}\,.\,[f.\tan\gamma_1 + c_1.cy_1.\sec^2\gamma_1]\,[f + c_1.cy_1.\tan\gamma_1]}$$

and if $\tan\gamma_1 = 0$ we get $\frac{W}{H} = \frac{550.5}{8} \cdot \sqrt{\frac{A.\rho}{W}}$

$q = 1$

$V_2/V_1 = 1$

$= .442 \times$ F. W. Lanchester's formula.

So that under similar ideal conditions ($\tan\gamma_1 = 0$, $q = 1$, $V_2/V_1 = 1$) we find that by making the inflow velocity constant over the blade we require only .442 times the B.H.P. required, to lift a given weight, as when using the "Rational" form of blade outline.

It is therefore obvious that for efficiency we require a blade form which is very wide near the boss and very narrow near the tips. Mr. Lanchester's condition of constant slip velocity is evidently an extremely sound one from the point of view of economy in B.H.P./weight ratio.

Returning then to the efficiency formula for an airscrew, having constant ($V_1$) and ($V_2/V_1$) values, we may estimate the B.H.P. required and the weight/B.H.P. possible under similar conditions to those assumed by Mr. F. W. Lanchester in his estimate already given, but taking into account the fact that in practice ($\tan\gamma_1$) is always greater than zero, and hence we may expect a result similar to the one already given by Mr. F. W. Lanchester, but not so optimistic.

This result is confirmed by an investigation of the analysis already established. However, owing to the fact that it is impossible to eliminate the factor ($c_1$) from the expression for weight/B.H.P. ratio, an exact comparison between F. W. Lanchester's expression and the one obtained on this theory is impossible.

It is also possible from the formulæ for weight/B.H.P. already given to find the greatest thrust possible with a given diameter propeller and given B.H.P.

We can write:—

$$\frac{W}{H} = 550 \cdot \sqrt{\frac{\left(1 + \frac{V_2}{V_1}\right) \cdot A.\rho}{W}} \text{ in lb. ft. sec. units.}$$

And putting

$$A = \frac{\pi}{4} \cdot d^2$$

we get

$$W = (8.27) \cdot \sqrt[3]{1 + \left(\frac{V_2}{V_1}\right)} \cdot d^{2/3} \cdot H^{2/3}$$

giving the equation required.

Thus, with a screw of 8ft. diameter and actuated by 1 B.H.P. the total thrust would be 41.7lbs. with ($V_2/V_1$) equal to 1, and 33.08lbs. with ($V_2/V_1$) equal to zero.

In the September number of "Aeronautics," 1911, the following figures are given for two screws tested by Professor W. H. Pickering, of Harvard:—

| | B.H.P. | Diameter of screw. | Thrust. |
|---|---|---|---|
| (1) | 1 | ... 12 feet ... | 48 lbs. |
| (2) | 20 | ... 20 ,, ... | 430 ,, |

Applying the above formula for the two cases when ($V_2/V_1$) is equal to 1 and when ($V_2/V_1$) is equal to zero to the above figures we get the thrust as calculated as given on the following table:—

| | B.H.P. | Diameter of screw. | Calculated Thrust. | ($V_2/V_1$)=1. | ($V_2/V_1$)=0. | Actual Thrust. |
|---|---|---|---|---|---|---|
| (1) | 1 | 12 feet | — | 54.6 lbs. | 43.3 lbs. | 48 lbs. |
| (2) | 20 | 20 ,, | — | 565 ,, | 448 ,, | 430 ,, |

These results appear to indicate that the purely theoretical formula established from R. E. Froude's theory is not so hopelessly out of agreement with the

facts as often happens to be the case when dealing with ideal conceptions of this kind.

Further particulars regarding the form of Professor Pickering's screws would have been interesting.

The formula here given for thrust is really only a variation of the one proposed by Mr. F. W. Lanchester in his paper "A Contribution to the Theory of Propulsion and the Screw Propeller" (Institution of Naval Architects, March, 1915), where his co-efficient ($Q$) has the value of:—

$$\frac{2}{1+\left(\frac{V_2}{V_1}\right)}$$

as given in this paper.

Probably a sufficiently approximate formula for the thrust delivered by a helicopter screw would be

$$W = K \,.\, (8.27) \,.\, \sqrt[3]{1+\left(\frac{V_2}{V_1}\right)} \,.\, d^{2/3} \,.\, H^{2/3}$$

where ($K$) depends upon blade form and may have an extreme value of unity. A graph of ($W$) against ($d$) is given in Figure 7, for $(V_2/V_1)=1$, $(K)=1$, and $(H)=1$.

## A COMPARISON OF THE NEW THEORY WITH PRACTICAL DESIGN RESULTS BASED ON THE OLD METHOD.

I now propose to discuss the differences which occur in practical design calculation between the results given by the new theory and those obtained by the old method employing correction factors.

It will be sufficient to outline a method based on the new theory whereby the correction factors may be calculated and the values of which may then be compared with those employed in drawing office design work.

Consider the calculation of the blade width constant on the new and old theories.

On the old theory the constant ($C$) is given by:—

$$C = \frac{550.H}{8.\pi^3.N.n^3.\rho.\int_{r_0}^{r} cy.f(x).x^3.\sec A.(\tan A+\tan\gamma)\,.\,dx}$$

and when this constant has been obtained in any given case it is usual in practice to multiply it by a correction factor ($1/f$), so that the real value of the constant then becomes ($c/f$).

Now on the new theory the blade constant ($c_1$) is given by:—

$$C_1 = \frac{550.H}{8.\pi^3.n^3.N.\rho.\int_{r_0}^{r} cy_1.f(x).x^3.\sec A_1.(\tan A_1+\tan\gamma_1)\,.\,dx}$$

and therefore if this new value ($C_1$) is assumed to be the correct value, no correction is required and hence we must have:—

$$C_1 = \frac{c}{f}$$

and $\therefore$

$$\frac{1}{f} = \frac{C_1}{C} = \frac{\int_{r_0}^{r} x^3 . f(x) . cy . \sec A . (\tan A + \tan \gamma) . dx}{\int_{r_0}^{r} x^3 . f(x) . cy_1 . \sec A_1 . (\tan A_1 + \tan \gamma_1) . dx}$$

and we may then obtain the value of the correction factor ($1/f$) in any given case considered.

This value appears to vary from about ($1/.75$) to ($1/.85$), and hence we may in this way form an estimate of the correctness or otherwise of the new process of design outlined in the paper.

I have found that, unless some allowance is made for the end effects on blades, the value of ($1/f$) obtained from this formula is usually below that given and of amount ($1/.9$) to ($1/.95$) approximately.*

A rough estimate of the value of ($1/f$) may be obtained by considering an element of blade at about ($\frac{3}{4}$) the extreme radius, and then we get:—

$$\frac{1}{f} = \frac{cy}{cy_1} \cdot \frac{\sec A}{\sec A_1} \cdot \frac{\tan A + \tan \gamma}{\tan A_1 + \tan \gamma_1}$$

for any radius considered, or preferably for a radius equal to about ($\frac{3}{8}$) the diameter of the propeller.

Considered from the point of view of actual propeller design work in the drawing office, the method here outlined presents greater arithmetical difficulties than the old process of design owing to the fact that, in order to determine the value of the blade constant ($C_1$), it is necessary to employ a somewhat tedious graphical process of solution.

Fig. 8 shows a graph of ($f$), plotted from the formula for an element already given, against radius for values of ($V_2/V_1$) of zero and unity. It will be noticed that as the radius increases the value of ($f$) falls and then rises again at the blade tip—a rise which I contend is probably due to neglecting the end effects analogous to the tip losses on aerofoils. The exact quantitative amount of such losses it does not seem to be possible to estimate at present, although further work in this direction may possibly enable some fixed percentage to be determined capable of being applied to most cases met with in practice.

The particular airscrew of which the above graph forms an analysis had a revolution speed in excess of the designed value, as might be gauged from the fact that the "two-dimensional" values of ($f$) on the curve are less than the values usually allowed for the correction factors in actual propeller design.

* Unless the blades are very wide, when the ratio becomes larger. It is really unsafe to generalise on this point, as it is quite conceivable that the correction factors commonly employed in the old theory are liable to variation—in which case the end effects on blades might be sufficiently small to be neglected entirely. Without wind-channel results it is extremely difficult to determine whether such fine points as these require modification or not. I think, however, that it may be taken for granted that narrow bladed airscrews require smaller correction factors than those with wide blades, quite apart from any possible end effects which may alter the character of the purely two-dimensional form of flow considered in the theory developed in the Paper.

The two other curves shown in Fig. 8 have been obtained by assuming a constant correction factor across the blade from boss to tip in conjunction with the two ($f$) curves already given in the same figure. These resulting curves form what may be considered a somewhat rough average of the end losses, boss and tip, to be taken into account in actual propeller design on the assumption that the correction factor necessary on the old theory remains a constant for all values of the radius.

Finally, an approximate formula has been deduced for the calculated correction factor ($f$) at any radius, in terms of the known quantities used on the old theory.

The formula obtained is:—

$$f = \frac{Cx + \dfrac{K.m.(a-\beta)}{K+m} \cdot \tan\left[A + \dfrac{m.(a-\beta)}{K+m}\right]}{Cx + Cy.\tan A}$$

where

$Cx$ = absolute drag co-efficient of section at ($a°$).
$Cy$ = ,, lift ,, ,, ,,
$a$ = angle of attack on old theory as already defined.
$\beta$ = ,, no-lift of section.
$K$ = a constant depending upon blade width and the $(V_2/V_1)$ value.
$m$ = ,, ,, ,, the characteristics of the section employed.
$A$ = the helix angle at any radius as already defined.

$(K) = (.1)$, $(m) = (.05)$ are ordinary values occurring in practice.
$(Cx)$ may have a value of about $(.03)$ depending upon the section of the blade.

An example shows the application of the above formula in the determination of ($f$) on the new theory.

Let

$$Cx = .03$$
$$Cy = .35$$
$$a = 4°$$
$$\beta = -3°$$
$$K = .1$$
$$m = .05$$
$$A = 12°$$

then

$$f = \frac{.03 + .233 \tan [12 + 2.33]}{.03 + .35 \times .213}$$
$$= (.857)$$

and therefore $(f) = (.857)$, so that $(1/f) = (1/.857)$, giving the calculated value of the correction factor required on the new theory.

In conclusion, the main points brought forward by the new theory, as an indication of possible directions in which enhanced efficiency may be expected to be found, appear to be as follows:—

(1) The importance of high aspect ratio, that is multiple blades.
(2) Good sections having high lift-drag values as aerofoils.
(3) Correct $(V/n.d)$ values for maximum efficiency.
(4) Wide blades should only be employed in conjunction with the correct chord angles for best efficiency as given by the methods here outlined.

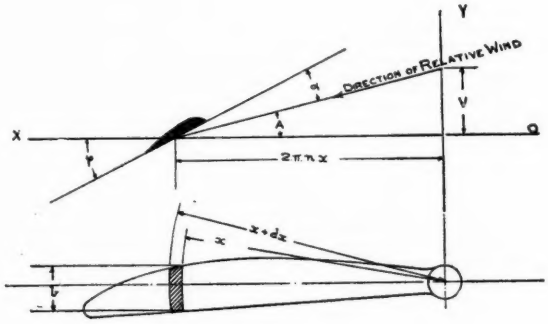



FIG. 1.

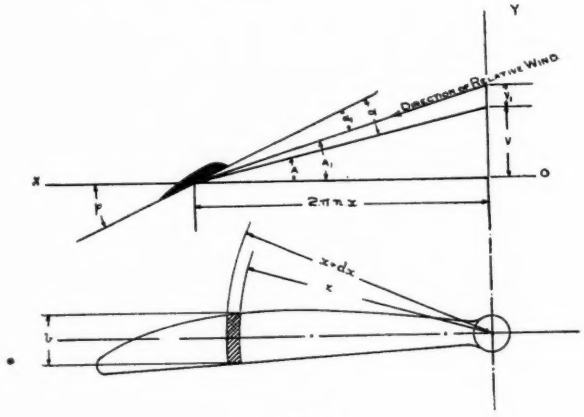



FIG. 2.

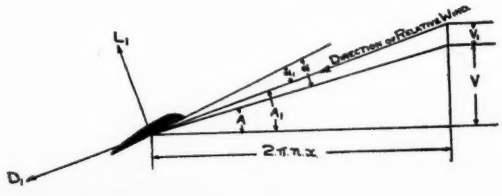



FIG. 3.

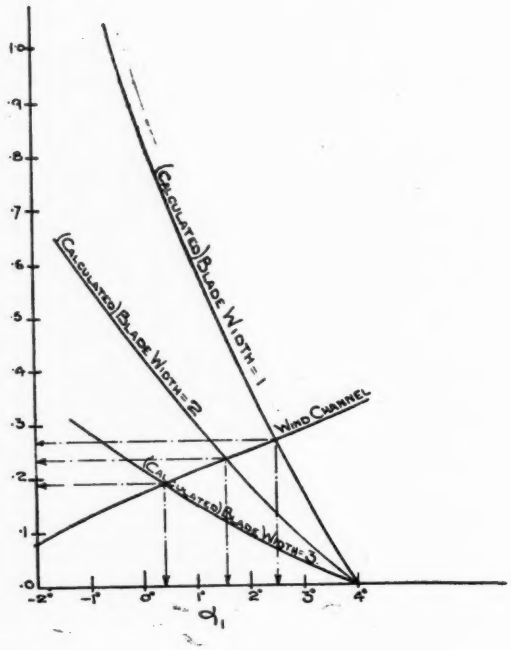

(Calculated) Blade Width = 1
(Calculated) Blade Width = 2
(Calculated) Blade Width = 3
Wind Channel
1·0
·9
·8
·7
·6
·5
·4
·3
·2
·1
·0
-2°
-1°
0°
1°
2°
3°
4°
α1

FIG. 4.

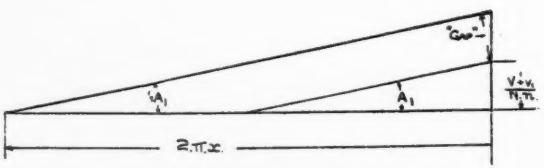

Cw
A1
2πx

FIG. 5.

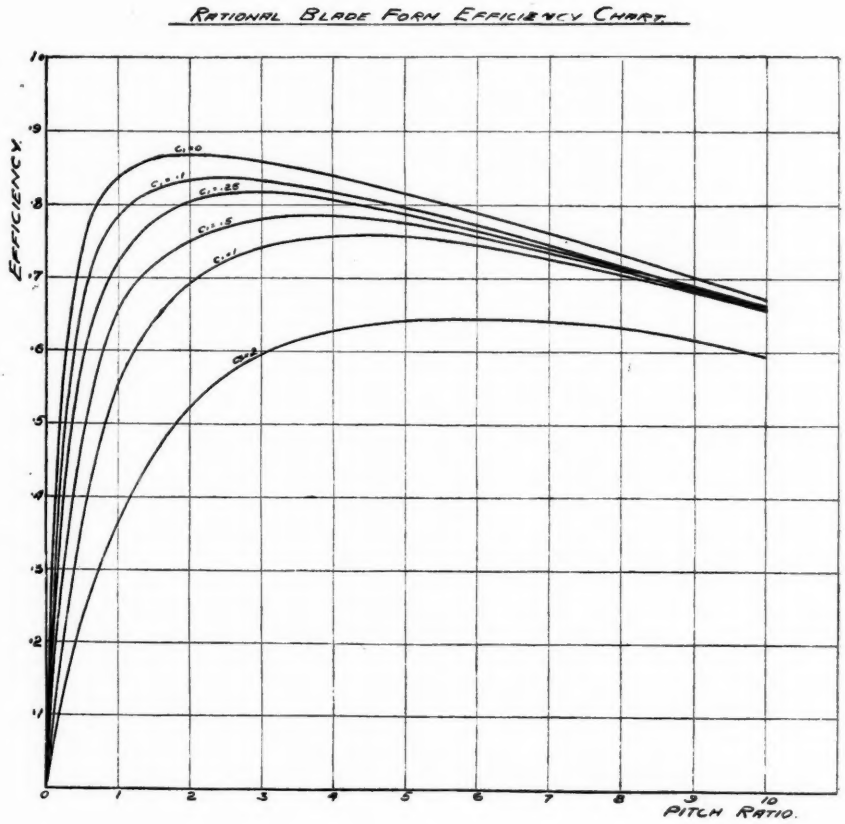

RATIONAL BLADE FORM EFFICIENCY CHART.
EFFICIENCY
C₁=.20
C₁=.1
C₁=.25
C₁=.15
C₁=.1
C=.2
PITCH RATIO.

FIG. 6.

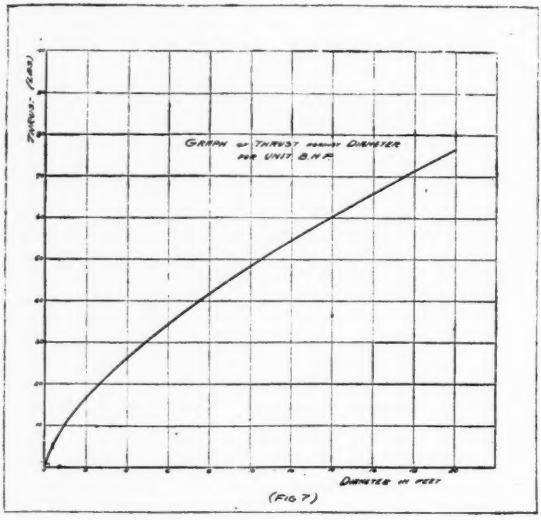

GRAPH OF THRUST PER UNIT DIAMETER PER UNIT B.H.P.
DIAMETER IN FEET
(FIG. 7.)

FIG. 7.

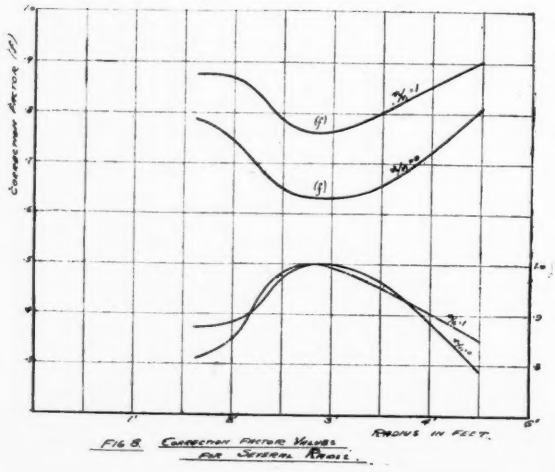

CORRECTION FACTOR (f)
RADIUS IN FEET.
FIG 8. CORRECTION FACTOR VALUES FOR SEVERAL RADII.

FIG. 8.

(5) There is still some uncertainty as to the correct amount of end tip loss to be allowed for, and further information regarding the $(V_2/V_1)$ values will probably be found to be necessary.

(6) Multiple bladed airscrews should, if the foregoing theory is correct, require more B.H.P. to turn them than their two-bladed equivalents under similar conditions, but they should develop a greater thrust at the same forward and rotational speed, and their efficiency should be higher.

(7) Multiple blades raises the question of the requisite strength and consequent area of the sections from boss to tip, which cannot be decreased indefinitely without fear of bursting taking place. Advantage may however be taken of the fact that a very slight forward rake is usually sufficient to convert the whole stress into a pure tension and consequently a much smaller section can then be employed. It should, however, be noted in passing that the centres of area of the blade sections along the blade should not in general lie on a straight line, but on a curve, the determination of which may be accomplished as a rule without much difficulty. All the sections will then be stressed in pure tension.

# THE HIGH-TENSION MAGNETO, WITH SPECIAL REFERENCE TO THE IGNITION OF AEROPLANE ENGINES.

BY A. P. YOUNG, A.M.I.E.E., A.I.A.E.

## PART I.

### (1) Foreword.

Early in 1915 I had the honour of delivering to one of the largest Engineering Societies in the Midlands, a lecture* on "The High Tension Magneto," which is—superficially at least—familiar to all those associated with the Aeroplane and Allied Industries. The present paper is really a continuation of my earlier effort, and, being presented to an Aeronautical Society, I shall naturally make special reference to the high-tension aeroplane type of magneto. The amount of technical literature on this subject available at the present moment is very meagre. It is therefore hoped that this contribution will have the effect of initiating a healthy discussion of ignition problems and stimulating the interest of all members of the engineering industry who are either directly or indirectly interested in the matter.

The degree of success achieved in the development of the combustion motor has at all stages been primarily dependent upon the efficiency of the ignition system used, and it is no exaggeration to say that the rapid strides which have, during recent years, been made in the construction of the petrol motor, *per se*, have mainly resulted from the very satisfactory high-tension ignition system that has been available. It is fundamentally true—although perhaps a mere commonplace—to say that the most perfect engine mechanism is of little use if the ignition system be faulty.

During the last two decades we have witnessed the birth and subsequent healthy development of high-tension ignition in the form of the magneto, and the wonderful efficiency of this system, coupled with its extreme flexibility—enabling one magneto to cope with almost any number of cylinders—is primarily responsible for the enormous developments that have taken place in the application of the petrol motor to industrial and—in more recent times—war purposes. Without the high-tension magneto it is almost certain that the aeroplane would never have reached its present high state of development, and—if I may be allowed to digress—it is reasonable to assume that the whole course of the present disastrous war would have been different, and much less in our favour—if the military critics are to be believed.

Prior to the outbreak of war the number of high-tension magnetos being produced in this country formed a negligible proportion of the total number being used for a variety of purposes. Through laxity on our part this most vital "key" industry was allowed to develop in Germany, but one is happy to know that the war has taught us a lesson in this respect and that our motto as a nation in the future will be "never again." During the period of the war the magneto industry in this country has developed at a really wonderful rate, and all those participating are to be congratulated—and indeed the country is indebted to them—on the present healthy and hopeful state of the industry.

* Lecture delivered to the Coventry Engineering Society on January 29th, 1915, and published in the "Automobile Engineer" for March, 1915.

You can judge what has been done when I state that the British Ignition Apparatus Association—which has been formed mainly as a result of the efforts of Mr. P. F. Bennett, the Chairman, and Mr. E. Garton, the Secretary—is at the moment supported by ten British magneto manufacturers who have, during the period of the war, supplied to the Government for war purposes, and chiefly for aeroplanes, no less than 200,000 magnetos. Furthermore, I think it is agreed by those in a position to judge that the British magneto as at present constructed is the equal of the pre-war Bosch magneto emanating from Stuttgart, which is sufficient testimony that the British manufacturers have done their duty. I can go further and state, from first-hand knowledge, that certain developments now taking place will result in the British manufacturers producing new types of magnetos that will prove to be vastly superior to anything turned out of Germany in the past.

I would therefore plead that when the war is over the British magneto manufacturers be given to the full encouragement and support by Government and public alike, so that there may be established on a solid foundation a British magneto industry that will endure through the years to come—an industry producing magnetos and ignition apparatus for aeroplanes, motor-cars, and other purposes, of superlative quality and design, not surpassed by anything manufactured outside of these little Isles.

**(2) Historical.**

All electrical systems of ignition are direct descendants of Faraday's great discovery of electro-magnetic induction made during the autumn of 1831, when for the first time in the world's history he succeeded in producing a spark by electro-magnetic means. He was experimenting with his classical iron ring wound with primary and secondary when he found, to his great joy, that by either closing or opening the primary circuit, it was possible to obtain a tiny spark between two carbon pencils separated by a very small distance and connected to the ends of the secondary.

The first system of electric ignition ever used was devised by Lenoir* in 1860. He utilised the high-tension spark of a Ruhmkorff coil for ignition purposes, employing a high-tension distributor for connecting the secondary winding first to one plug and then to the other. It is worth noting that the modern battery system of ignition now used most extensively in America is strikingly similar to the old Lenoir system, even to the detail of introducing an extremely small air gap between the rotating metal brush and the distributor segment—a method of distribution that is now being followed on magnetos. Lenoir was also the inventor of the spark plug, and the type of plug which he designed for use in conjunction with his ignition scheme of 1860 had all the features of the modern spark plug.

Marcus appears to have been the first man to construct a magneto for ignition purposes. His was a low-tension machine, having the now familiar form of H armature, the current induced in the winding being broken at predetermined times in the cylinder by a system of cams and levers.

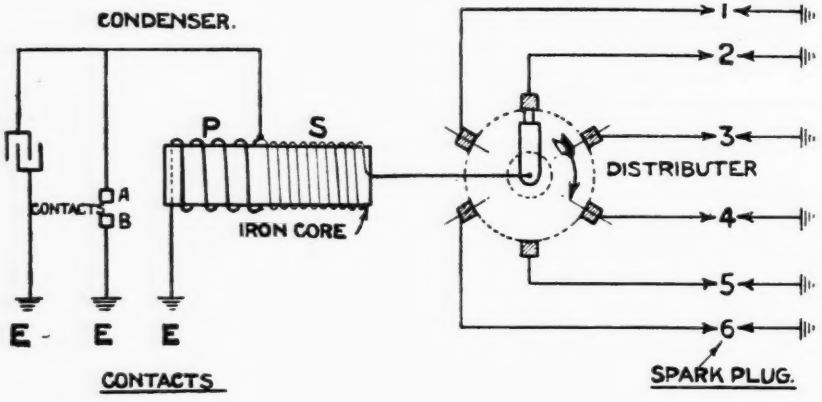
In 1898 Simms and Bosch developed a low-tension magneto, using a fixed H armature and rotating segments for producing the necessary flux changes in the armature core. This is of special interest, because subsequently, by the addition of a secondary winding on the same armature core, a high-tension magneto was evolved. Later on I shall have occasion to describe in considerable detail a magneto of this particular type which is being built in very large quantities for aeroplane work.

The Bosch Company, of Stuttgart, Germany, must receive the credit of

* See Appendix 1 and Fig. 40, showing this system.

having thoroughly established the fact that a high-tension magneto can be manufactured on a commercial basis, to give reliable and efficient ignition in practice. Although this important industry was allowed to develop in Germany, the modern H.T. magneto was first conceived in France by the Frenchman M. Boudeville,† who unfortunately omitted to include a condenser in his scheme for eliminating sparking at the contact points.

A condenser is a vital part of every magneto; without it the machine would be quite impracticable. It is surprising that Boudeville should have overlooked this feature, because here again the idea of using a condenser for such a purpose is of French origin, the Frenchman Fizeau being the first to suggest, in 1853, connecting a condenser in parallel with the contacts on a Ruhmkorff coil, to prevent excessive sparking.




NOTE:-

FOR CONVENIENCE, A 6 CYLINDER DISTRIBUTER IS ILLUSTRATED. IN GENERAL, THE NUMBER OF DISTRIBUTER SEGMENTS IS EQUAL TO THE NUMBER OF CYLINDERS.

FIG. 1.

### (3) Fundamental Considerations.

Any form of high-tension magneto consists of four essential parts:—

(1) A magnet system.
(2) An iron core wound with primary and secondary.
(3) A contact breaker to interrupt the primary circuit at predetermined intervals.
(4) A high-tension distributor.

The arrangement of connections generally adopted is clearly illustrated in Fig. 1. The earthed primary contact (B) is actuated by some form of cam so that the primary circuit is continually being closed and opened.

The iron core which carries the armature windings may either revolve or be fixed. In the latter case the rotor comprises certain iron masses so disposed that

† See "Magneto for Electric Ignition," by H. Armagnat, in "La Revue Electrique"—Translation in "The Electrician," March 24th and 31st, 1916—which deals fully with the history of Electric Ignition.

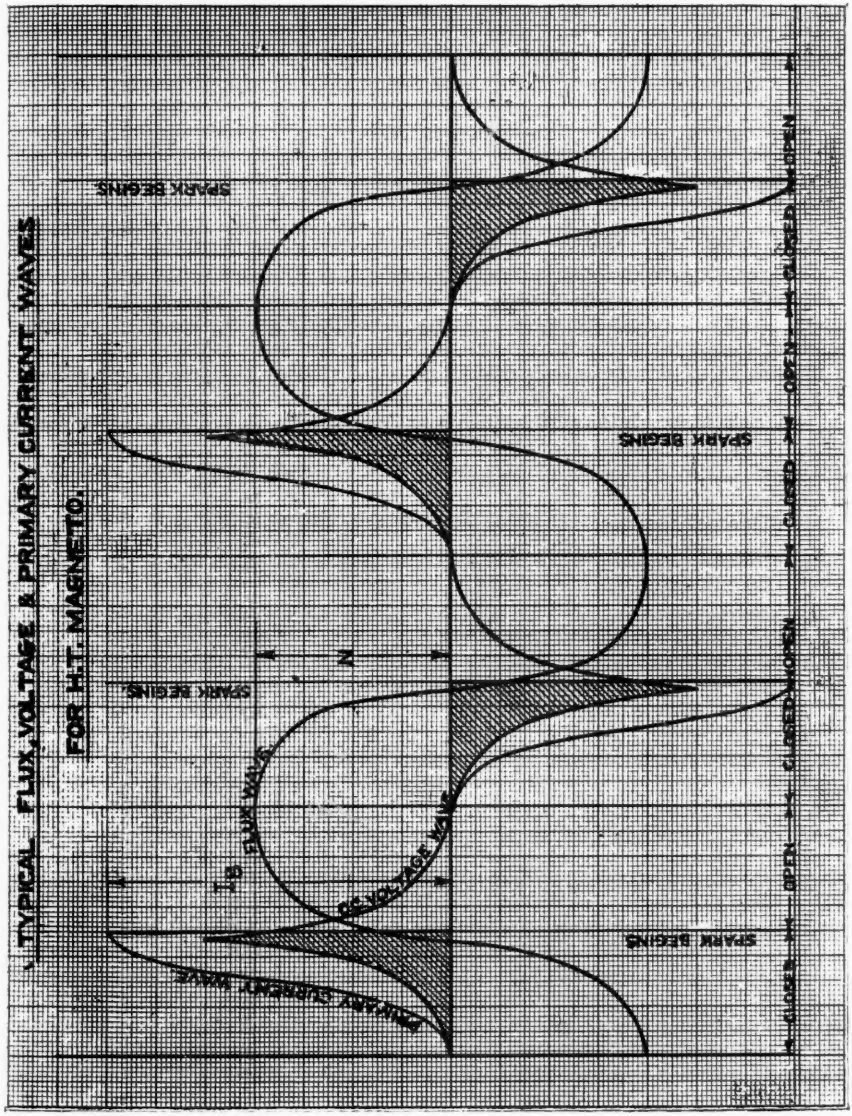

TYPICAL FLUX, VOLTAGE & PRIMARY CURRENT WAVES
FOR H.T. MAGNETO.
SPARK BEGINS
SPARK BEGINS
SPARK BEGINS
SPARK BEGINS
FLUX WAVE
VOLTAGE WAVE
PRIMARY CURRENT WAVE
CONTACT OPEN
CONTACT CLOSED
CONTACT OPEN
CONTACT CLOSED

FIG. 2.

every 90° or 180° (depending upon whether the magneto is a two-spark or a four-spark), the flux flowing from the N pole of the magnet to the S pole, through the armature core, is suddenly reversed.

When the armature rotates the flux reversal occurs—except when specially-shaped poles are fitted—every 180°, and the magneto gives two sparks per revolution.

Magnetos provided with stationary armatures are said to be of the inductor type. To prevent confusion we shall adopt the following nomenclature :—

(1) When the rotor comprises iron segments built in the form of a sleeve, which revolves around the stationary armature, we shall call the machine a " Sleeve Inductor " type magneto.

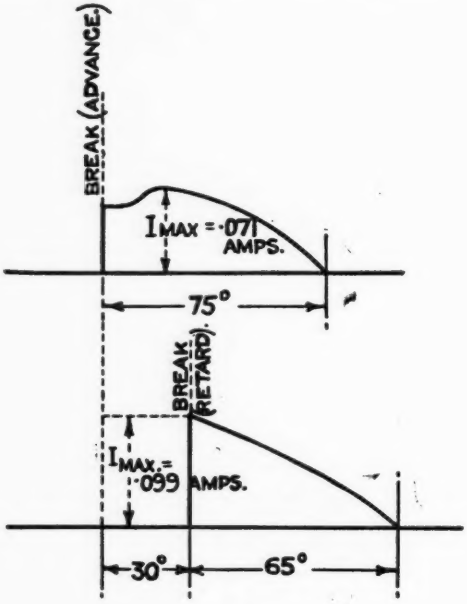



FIG. 3.

(2) When the rotor comprises iron masses operating in conjunction with an external armature core, we shall call the machine a " Polar Inductor " type magneto.

(3) When the armature rotates—as is more usually the case—we shall call the machine a " Rotating Armature " type magneto.

Dealing first with the problem in the abstract, we give below a description of the cycle of operations that is applicable to any particular construction. Reference should be made to Fig. 2, which shows a typical flux wave, and the resultant open circuit voltage and primary current waves.

(*a*) The rapid reversals of flux in the armature core induce in both primary and secondary an alternating E.M.F., the maximum E.M.F.

being reached for any given speed, when the actual flux in the armature core is substantially zero.

(*b*) The cam operating the contact breaker lever is so designed that the primary circuit is closed during the period when the induced voltage is growing from zero to its maximum value, and for some little while afterwards, but not until the voltage has become zero again.

(*c*) During the period of closure of the primary circuit the induced current will steadily increase in value. Then at a certain instant the contacts are suddenly separated by the cam. The condenser in parallel with them ensures that there is no sparking at this instant, so consequently the rupture of the primary current is remarkably sudden.

(*d*) The sudden rupture of the primary current causes an instantaneous collapse of the magnetic field associated with it, and as this field is linked with the secondary turns, an enormous voltage is induced in the secondary winding at this instant. This voltage is sufficient to initiate a spark between the electrodes of the spark plug, and this spark is maintained, to some extent, by the voltage induced in the secondary by virtue of the continual flux change in the armature core produced by rotation during the period that the contacts remain open. (See Fig. 3.)

(*e*) After a certain short interval of time the cam allows the contacts to close again, and the cycle of operations (*b*) (*c*) (*d*) is repeated.

**(4) Analysis of the Working of a Magneto.**

From the foregoing description of the cycle of operations we can easily set down the factors which govern the operation characteristics of any H.T. magneto :—

Maximum value of flux through armature core $= N$.
Angle during which contacts remain closed (measured on revolving shaft) $= \theta_1$.
Angle during which contacts remain open $= \theta_2$.

$$\text{Ratio } \frac{\theta_1}{\theta_2} = k$$

No. of primary turns $= S_1$.
No. of secondary turns $= S_2$.

$$\text{Ratio} = \frac{S_2}{S_1} = k_1$$

Value of primary current at "break" $= I_B$.
Self-induction of primary at "break" $= L$.

Other things being equal, the voltage induced in the secondary winding at "break," and the amount of energy liberated in the H.T. spark, are dependent on $I_B$. The best results are therefore obtained when, for any speed over a very wide range, $I_B$ corresponds to the maximum point on the primary current wave. Unfortunately, this result cannot be achieved in practice, because if $I_B$ is a maximum at low speeds, the current broken at high speeds (for the same timing) will be less than the maximum, and *vice-versa*.

Theoretically, the whole of the energy stored up in the primary winding during the periods when the contacts are closed and the primary current is creating a magnetic field should reappear in the secondary spark at "break" if there were no losses. This energy, which is stored in the primary at a low potential, is, as it were, suddenly thrust into the secondary at "break," where it

reappears at a high potential. Actually, certain losses occur during the transfer, and only a portion of the energy stored magnetically (amounting to $\frac{1}{2}LI_B^2$ Joules) appears in the H.T. spark. Generally speaking, about 80% of this energy is available for ignition in a well-designed magneto.

The problem, then, is to determine the value of $I_B$. If we know the formation of either the flux or voltage wave as given in Fig. 2, this can be easily done. The flux wave can be deduced experimentally by what is called a "point to point" method. That is, a fine search coil is wound on the armature windings (or the primary itself may be used if the turns are known) and connected to a ballistic galvanometer. By moving the rotor through a large number of consecutive and equal angles (say 15°), noting the deflection of the galvanometer corresponding to each movement, the corresponding flux changes can be calculated and from these figures the flux wave deduced.

Having a flux wave, the E.M.F. wave can be calculated from the formula :—

$$e = \frac{-S_1}{10^8} \frac{dN}{dt} \text{ volts} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Another method is to determine the E.M.F. wave on open circuit, experimentally, by using an oscillograph, and calculate the flux wave by applying this same formula re-written as follows :—

$$N = \frac{-10^8}{S_1} \int e.dt \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Assuming that the E.M.F. wave for any given speed is known, the value of $I_B$ can be determined by the graphical method which I have developed elsewhere.* I shall recapitulate.

In general, we can represent the growth of the primary current during the period when the contacts remain closed by the following fundamental formula :—

$$e = ri + L \cdot \frac{di}{dt}$$

Where,

$e$ = Instantaneous voltage induced in primary circuit as determined from the curve, such as that shown in Fig. 2.

$i$ = Instantaneous value of the current in the primary. Amps.

$r$ = Resistance of primary. Ohms.

$L$ = Self-induction of primary at the instant under consideration. Henries.

Integrating this equation we get :—

$$L\,({}^iT_2 - {}^iT_1) = \int_{T_1}^{T_2} e.dt - r. \int_{T_1}^{T_2} i.dt$$

If $(T_2 - T_1) = T$ is small, then we get approximately :—

$$L\,({}^iT_2 - {}^iT_1) = X - r.T \frac{({}^iT_2 + {}^iT_1)}{2}$$

Where

$X$ = Area of voltage wave included between $T_1$ and $T_2$.

Finally,

$${}^iT_2 = \frac{2X + {}^iT_1\,(2L - rT)}{r.T + 2L} \quad . \quad . \quad . \quad . \quad (3)$$

* "The H.T. Magneto," by A. P. Young, "Automobile Engineer," March, 1915.

By using this formula and applying a point-to-point method it is a fairly easy matter to determine the primary current curve (and therefore $I_B$) for any type of magneto at any speed, provided that the constants of the design are known. Later on I shall refer to a series of curves for a sleeve inductor type of aeroplane magneto that have been deduced by this method.

It is possible to calculate $I_B$ directly by making certain justifiable assumptions, and in one of the appendices to this paper I have developed a formula which can be used for such calculations.

Referring to equation (3), it should be noted that the factor X is proportional to the flux change in the armature core, due to rotation, that occurs in the short period of time considered. It is therefore easy to see that the current $I_B$, for any given design, is within limits proportional to the maximum armature core flux N, this in turn being dependent on the magnetic characteristics and design of the magnet (or magnets) used. As the energy in the high-tension spark is proportional to $(I_B^2)$, we must conclude that the energy liberated at each discharge (that is, the intensity of the spark, but not necessarily the power of the spark to produce ignition) is proportional to the *square of the maximum value of the flux produced in the armature core on open circuit by the system of magnets used.*

It is therefore evident that the magnet flux is one of the controlling factors in the design of any magneto, and very careful attention has to be given to the magnetic quality of the magnets and the design of the whole magnetic system to ensure that the best results are obtained. I shall deal more fully with this aspect of the problem under the heading "Magnets—Importance of Flux."

### (5) The Phenomena that occur in the Secondary at "Break."

(*a*) *Induced Secondary Voltage.*

Reference should be made to Fig. 4, which shows by means of a curve the way in which the flux flowing in the armature core and linked with the windings changes at the instant of "break." Actually, the flux reverses its direction, because so soon as the magnetic field associated with the current $I_B$ disappears, the magnet flux is reinstated. The effect on the secondary is equivalent to the sudden destruction of the flux associated with the current $I_B$, and in what follows we shall view the problem in that way.

It is therefore clear that prior to "break" a certain amount of energy is stored in the primary winding by virtue of the magnetic field associated with the current induced in that winding by rotation. The magneto is designed so that at the instant when the contacts separate this energy can be transferred magnetically to the secondary winding to reappear at the points of the spark plug in the form of a high-tension discharge at very much higher potential.

The rate at which the secondary voltage rises, and the value of the maximum reached, are dependent on the primary current broken ($I_B$); on the self-induction of the primary at "break"; on the ratio of turns $k_1$; and on the characteristics of the magnetic circuit. In other words, it is necessary that the magnetic field created by the primary current shall instantly collapse at "break." Obviously, the rate at which the magnetic field disappears is, other things being equal, vitally dependent upon the characteristics of the magnetic circuit through which this magnetic field is passing. By this I mean that a magnetic circuit which is totally laminated will ensure a more rapid dying away of the magnetic field than will occur if there are in the magnetic circuit any solid masses in which eddy currents may be set up during the period of change, these eddy currents, by their reaction, tending to retard the rate at which the collapse of the magnetic lines occurs.

The character of the secondary voltage wave during the period of the high-tension discharge is of considerable importance, because the maximum value reached would seem to have some influence on the power of the spark to produce

ignition. Furthermore, the shape of the wave controls, to some extent, the rate at which the energy liberated in the high-tension spark is dissipated, and I hold the view that the rate of energy discharge is of more importance than the total

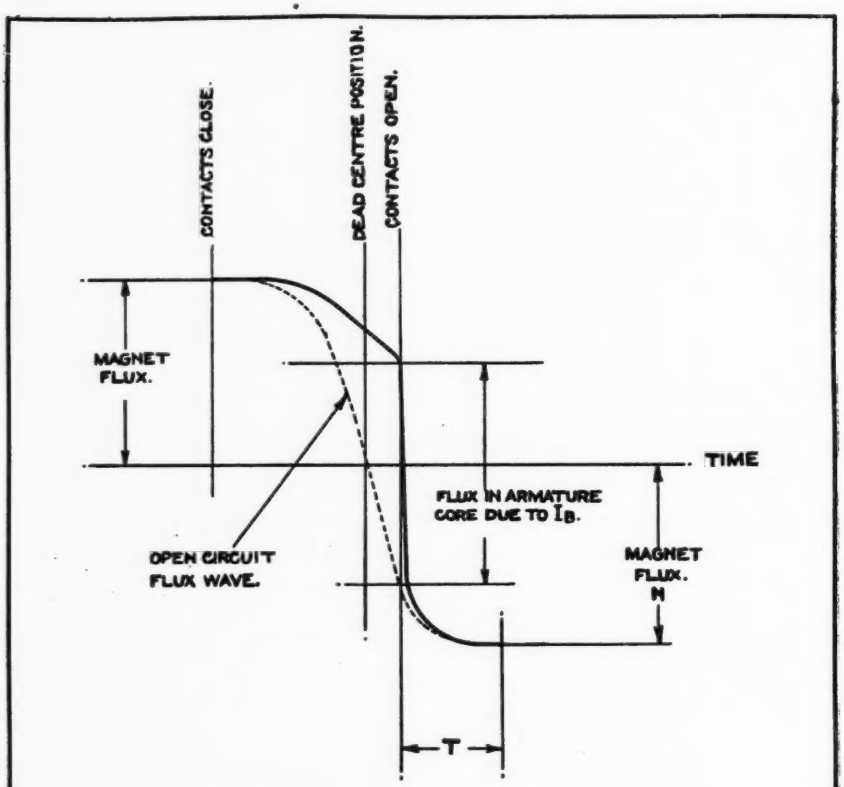

CONTACTS CLOSE.
DEAD CENTRE POSITION.
CONTACTS OPEN.
MAGNET FLUX.
TIME
FLUX IN ARMATURE CORE DUE TO $I_B$.
OPEN CIRCUIT FLUX WAVE.
MAGNET FLUX. M
T
NOTE:-
GENERALLY, THE CONTACTS SHOULD REMAIN OPEN FOR A PERIOD OF TIME NOT LESS THAN T.
CURVES SHOWING FLUX CHANGE IN ARMATURE CORE PRIOR TO, & IMMEDIATELY AFTER "BREAK."

FIG. 4.

amount of energy liberated, although the latter factor must be taken into consideration.

It is a very difficult matter to investigate this point quantitatively, but experiments (see Fig. 5) made on magnetos, using a special valve known as a

Kenotron,* which has been developed in one of the largest Industrial Research Laboratories in America, indicate that the voltage rise in the primary winding at the instant of " break " is very much greater than one would anticipate. It is reasonable to suppose from these experiments that a correspondingly abnormal rise of voltage occurs in the secondary at " break "—this voltage, of course, only being maintained for an infinitesimal period of time—and the curve shown in Fig. 6 represents the way in which I imagine the secondary voltage varies during the period of the high-tension spark. I have not so far been able to determine this curve experimentally, but taking certain indirect evidence in my possession, this curve represents, so far as I can see, what actually occurs.

It will be noted that I assume the voltage suddenly rises to a maximum and then suddenly falls again, the rise and fall occupying a time $T_1$, during which period there is no actual discharge between the electrodes, but the gases in the vicinity are being ionised preparatory to a discharge which occurs at the point " A " and is maintained during the subsequent period $T_2$, at the end of which the voltage has dropped away to zero. The voltage corresponding to the maximum point on the curve I will call the " maximum voltage," whereas the voltage at

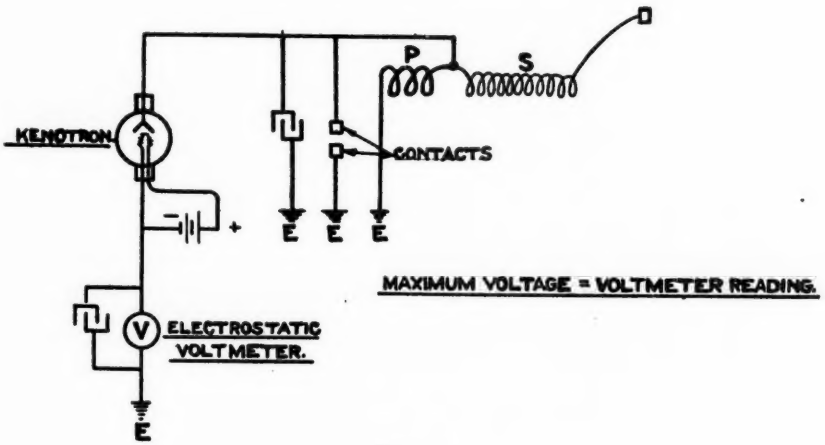



FIG. 5.

the beginning of the discharge period $T_2$ I shall call the " sparking voltage." As soon as the spark is initiated at the point " A " the discharge is maintained to some extent by the voltage induced in the secondary winding in consequence of rotation, and this voltage is virtually superimposed upon the voltage induced as a result of the collapsing of the magnetic field stored in the primary prior to break.

*(b) The Process of Ignition.*

Unfortunately, the present state of our knowledge† concerning the process of ignition by means of a high-tension spark is by no means complete, but I rather suspect that the maximum value of the secondary voltage reached is a controlling factor, and it is even within the bounds of possibility that the ionisation occurring

* For a detailed description of the theory and working of the Kenotron, see article in " General Electric Review " for March, 1915.

†An excellent article on " Spark Ignition," by J. D. Morgan, A.M.I.C.E., A.M.I.E.E., appeared in " Engineering " for November 3rd, 1916. See also numerous papers by Dr. W. M. Thornton in Proceedings Roy. Soc. and Phil. Mag.

as a result of the sudden application of an enormous voltage during the extremely small interval of time $T_1$ may in itself be sufficient to initiate the explosion. It has been demonstrated* that ionisation in itself will produce an explosion even though no spark occurs, and I am inclined to the opinion that the apparent superiority of the polar inductor type of magneto over the rotating armature type is due to the fact that the secondary voltage wave is steeper and reaches a higher maximum during the interval $T_1$, in consequence of the armature circuit being more thoroughly laminated and therefore allowing of a more rapid flux change.

The process of ignition by high-tension spark is a subject worthy of Government research, and if in the future action is taken in this direction, I would

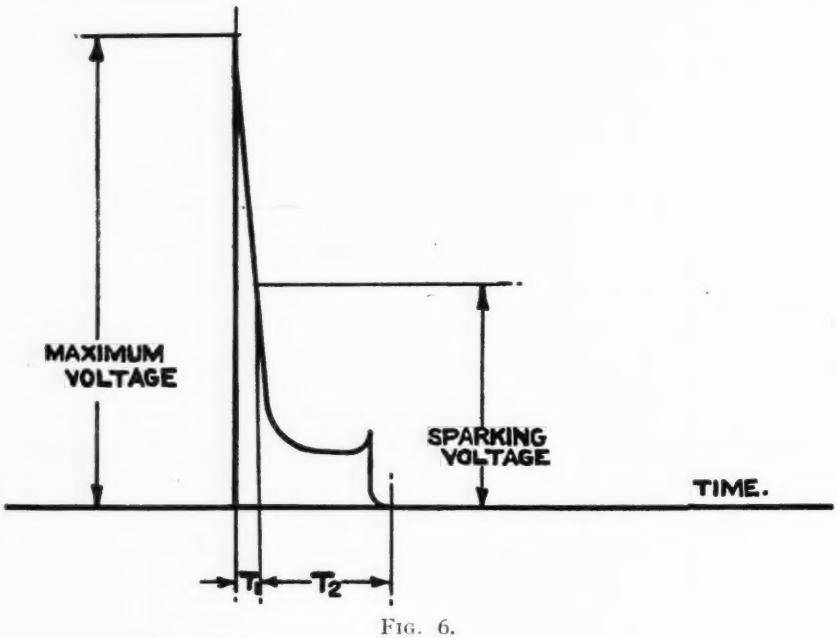



FIG. 6.

suggest that experiments be conducted with magnetos of both the polar inductor, and rotating armature types, with a view to ascertaining to what extent the process of ignition is dependent upon the shape of the secondary voltage wave and whether, as suggested above, the ionisation that must occur during the initial period $T_1$ is a predominant factor.

*(c) The "Spark-Gap" type of Distributor and Brush.*

When discussing this question it is not out of place to consider what is termed the "spark-gap" form of distributor. A common trouble experienced with the ordinary type of distributor in which a carbon brush is used is that the carbon dust generated is liable to be ground into the brush track of the distributor, thus producing a path of low resistance between the segments, this disease being known as "tracking." In certain cases, and particularly when two six-cylinder

* See "Notes on the Ignition of Explosive Gas Mixtures by Electric Sparks," by J. D. Morgan, A.M.I.C.E., A.M.I.E.E., in the Journal of the Institution of Electrical Engineers, Vol. 54, January 15th, 1916.

magnetos are used in conjunction on a 12-cylinder engine, tracking of this kind is very liable to cause misfiring, because the spark, rather than discharge between the electrodes of the spark plug in the cylinder under compression, will leap along the surface of the brush track in the direction of rotation to the next segment which is connected to a spark plug in a cylinder at practically zero pressure, the resistance of this alternative path being extremely low. (See Fig. 7.)

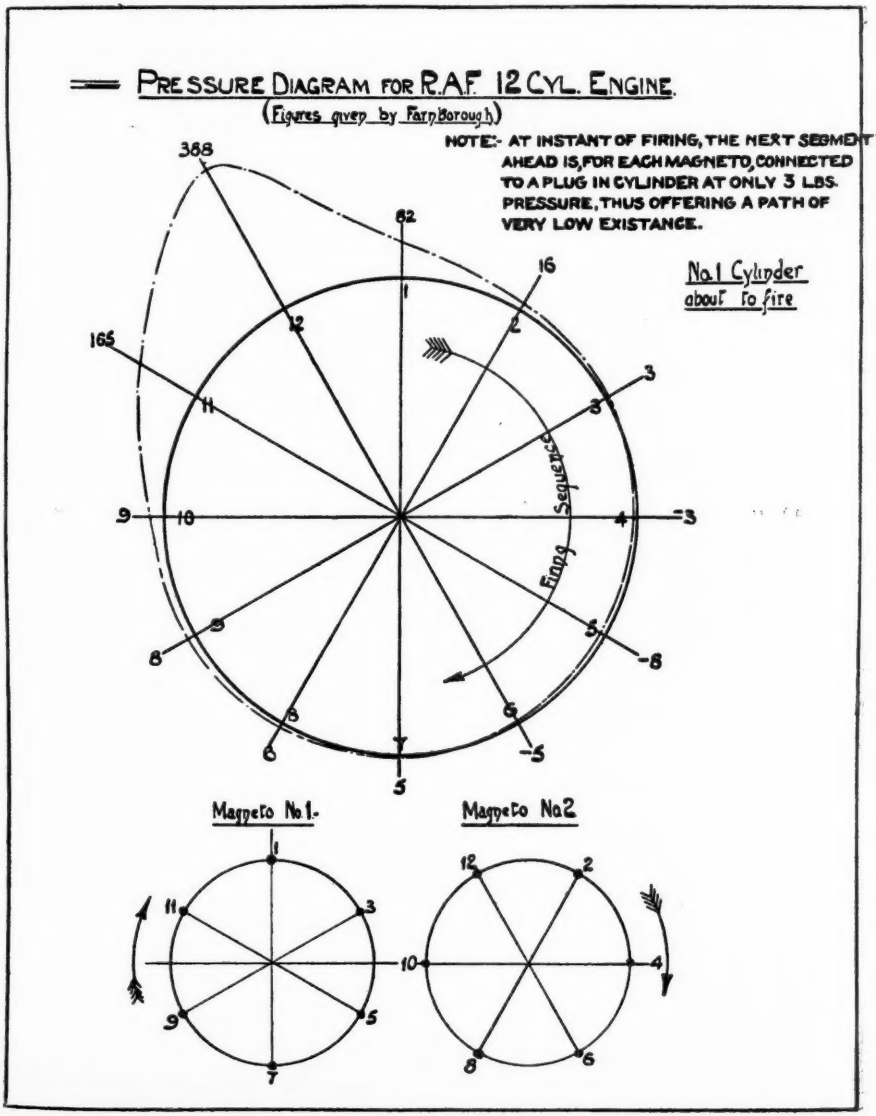

PRESSURE DIAGRAM FOR R.A.F. 12 CYL. ENGINE.
(Figures given by Farnborough)
NOTE:- AT INSTANT OF FIRING, THE NEXT SEGMENT AHEAD IS, FOR EACH MAGNETO, CONNECTED TO A PLUG IN CYLINDER AT ONLY 3 LBS. PRESSURE, THUS OFFERING A PATH OF VERY LOW EXISTANCE.
No.1 Cylinder about to fire
Firing Sequence
Magneto No.1.
Magneto No.2

FIG. 7.

Experiments have shown that even with a distributor that is badly tracked misfiring can be entirely eliminated if, instead of the ordinary carbon brush, a metal brush be used, this being so arranged that there is a very small air gap (about 0.01in.) between the face of the rotating metal brush and the distributor segment. The superiority of this arrangement over the distributor and carbon brush combination would seem to be due to the fact that the introduction of a small air gap in the high-tension circuit, such as is interposed between the end of the secondary winding and the outside circuit, completely insulates the secondary during the small interval of time $T_1$, and thus allows the voltage to rise to a very much higher maximum value than would be the case if there were any leakage paths of low resistance.

This is a further confirmation of the view that the value of the maximum value of the secondary voltage at " break " is of vital importance, and I would add that the spark-gap form of distributor has been tried with considerable success on aeroplane magnetos both in this country and in France, and, as a matter of fact, it is used generally on all the battery ignition systems designed for automobile work in use in America. In my opinion it would seem to be an advance in the right direction provided that sufficient care is given to the design of the brush holder and proper precautions are taken to ventilate the distributor so that the products of ionisation resulting from the minute spark between metal brush and segments have easy means of escape. This has been done in the type " A " magneto, which I shall describe in detail later, and many thousands of these machines fitted with this special form of distributor brush have been supplied for use on aeroplanes, and the consensus of opinion seems to be that the arrangement adopted is entirely satisfactory.

**(6) Magnets—Importance of Flux.**

The function of the magnets is to produce and maintain, in spite of the very severe conditions under which a magneto has to operate—including severe vibration and cyclic changes in temperature—a practically constant flux in the armature core. I have already shown that the energy liberated in the high-tension spark is substantially proportional to the square of this flux, so that the necessity of using magnets of correct design and superlative quality is at once apparent.

For any given machine using magnets of some definite design the value of the armature core flux " N " is naturally dependent on the magnetic characteristics of the magnets used. In comparing permanent magnets of different quality, it is usual to determine the remanence ($B_R$) and the coercive force ($C_F$). These factors apply to a closed ring of the steel which is capable of being completely and uniformly magnetised. By varying the current in the magnetising coil, the steel can be taken through a cycle of magnetisation, as depicted by the hysteresis loop shown in Fig. 8. What is meant by remanence and coercive force can be better appreciated by referring to this diagram. The remanence is the flux density in the steel, after the magnetising force Hmax (usually 400) has been removed, and the coercive force is the demagnetising force that has to be subsequently applied to reduce this flux density to zero. When testing a magneto magnet, the ideal " ring " condition can be approximated by bridging the poles with a soft iron keeper and suitably winding the magnet. A fundamental test of this kind is, however, attended with considerable difficulties.

It is the practice of the B.T.H. Company, and, I believe, of most other magneto manufacturers in this country, to test every magnet by means of certain special apparatus before it is fitted to a magneto, to determine both of these factors. So far as we are concerned, we accept only those magnets which show on this magnetic test a remanence and coercive force in conformity with the following :—

(1) The product of remanence and coercive force in C.G.S. units must not be less than 580,000.

(2) The actual coercive force must not be less than 55.

Experiment has shown that the active armature core flux in a magneto of given design is greatly dependent on the product of remanence and coercive force, or upon what I shall term the "magnet strength," using this expression to denote the factor $\frac{(B_R \times C_F)}{10^4}$. The spark energy output, being proportional to the square of the armature core flux, must in consequence be dependent on the magnet strength to a much greater degree.

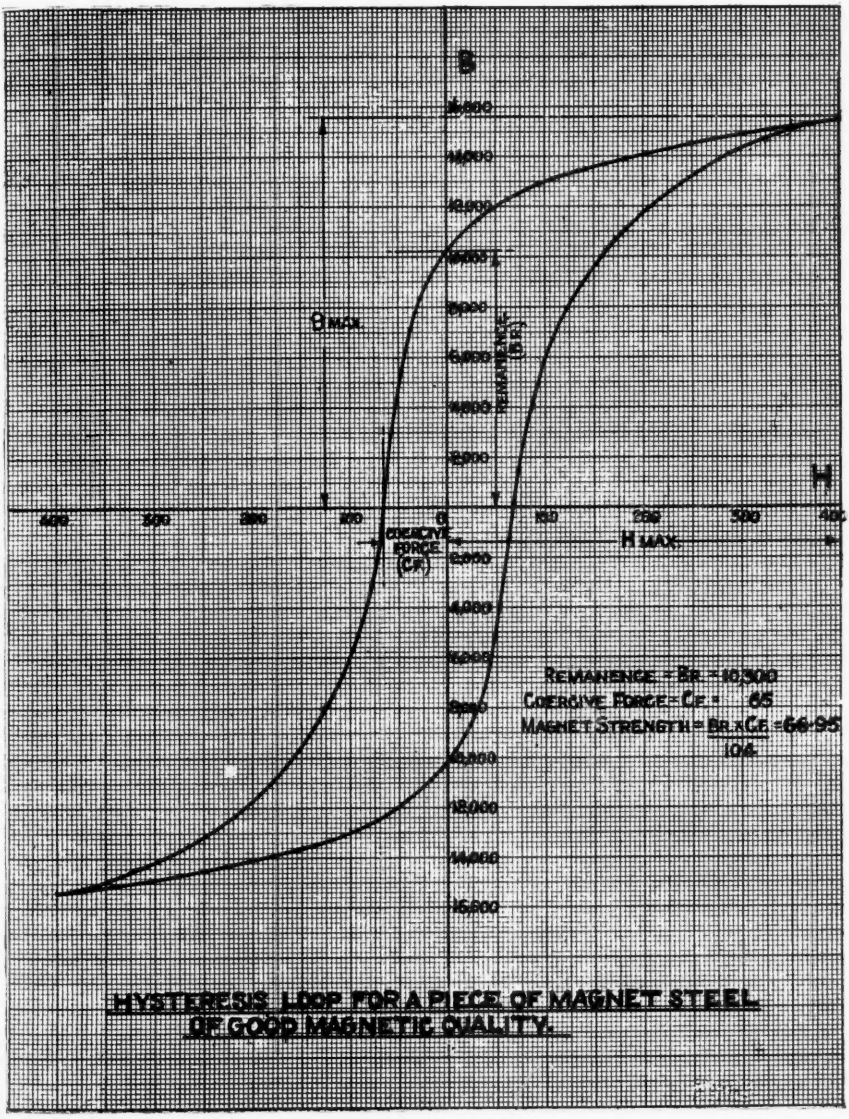



FIG. 8.

The product remanence and coercive force is therefore a very important factor, and, generally speaking, it is sufficient when testing magneto magnets to measure this product only, because as a general rule a magnet which shows a product of not less than 580,000 will have a coercive force not less than 55. In other words, such a relatively low coercive force as 55 is usually associated with a remanence of the order of 10,500.

Looking at a magneto magnet from a purely mechanical standpoint, it is necessary that the pole faces, and also the edges, should be finally ground after hardening to a fairly fine degree of accuracy. It is of great importance that the ground pole faces be parallel to each other and exactly at right angles to the ground edges. Some idea of the degree of accuracy required can be gathered from the following limits, which are now being worked to by Sheffield manufacturers :—

Distance between the ground pole faces—Maximum + .2 mm.
" " " " " —Minimum — .2 mm.
Width of magnet—Maximum + .15 mm.
" " " —Minimum — .15 mm.

These limits may at first sight appear to be large, but it should be remembered that a hardened Tungsten steel magnet is a very difficult thing to grind, particularly when it comes to grinding the inside pole faces, because trouble is encountered, due to the springiness of the magnet itself.

Before leaving this aspect of the problem, I should like to make some reference to the very excellent way in which the various Sheffield steel manufacturers now engaged on the manuacture of permanent magnets have carried out their work. Bearing in mind that at the outbreak of war practically no permanent magnets were produced in this country, and that this subsidiary key industry was entirely in the hands of Germans, I think that all those who have been closely in touch with the development of a magnet industry in this country will agree that the results so far achieved are very gratifying, and reflect great credit on the British steel manufacturers, who, by virtue of their resourcefulness and skill, have grappled so successfully with this difficult problem. Our own experience is that at the present time permanent magnets for magnetos are being produced in this country which are quite equal—and in many cases superior—to anything that has been produced in Germany in the past, and I am glad to know that Mr. E. A. Watson, Technical Director of the M.L. Magneto Syndicate, Ltd., holds views* which coincide with my own in regard to this important question.

## PART 2.

## AEROPLANE TYPE MAGNETOS.

### (1) General.

The greater number of the magnetos built in this country since the outbreak of war have been used on aeroplanes. This means that the British magneto industry, from the moment of its inception, had to contend with the most difficult problem of all, because it is generally recognised that an aeroplane magneto has to operate under more severe and exacting conditions than obtain on any form of motor vehicle.

The mantle of responsibility was borne during the early stages of the war by the firm of Thomson Bennett, in Birmingham (who, to their credit, were struggling to create a magneto industry before the war); the B.T.H. Co., in Coventry; and the M.L. Magneto Syndicate, in Coventry. More recently many newcomers have joined the industry, with the result that at the present moment there are no less than 15 manufacturers engaged on this most important work.

* See letter in " Autocar " for February 12th, 1915, page 215.

Special credit is due to the three manufacturers named above, and in particular I should like to refer to the most excellent work that has been done by Mr. E. A. Watson, who is Technical Director of the firm of the M.L. Magneto

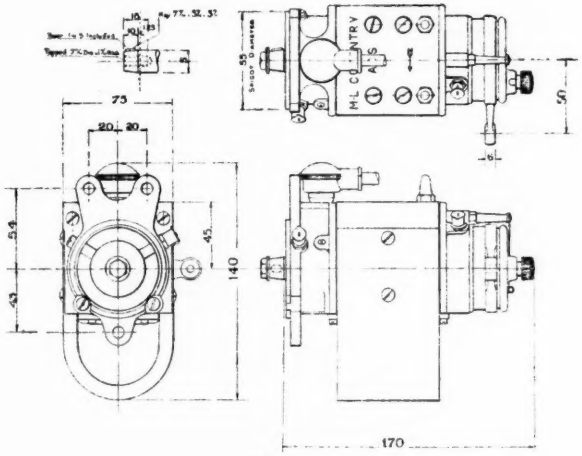



FIG. 9.

Syndicate, on the technical side of the business. Mr. J. D. Morgan, of the Marks and Clark Laboratory and Consulting Engineer to the firm of Thomson

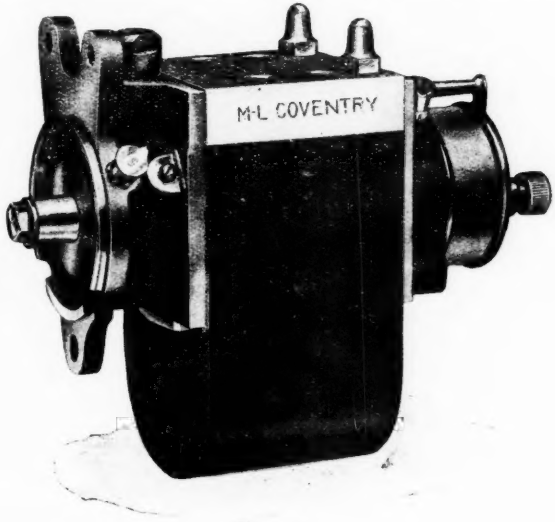



FIG. 10.

Bennett, has also done a large amount of research work, more particularly in connection with the process of ignition by electric spark.

As evidence of what has already been done, I propose to describe some of the aeroplane type magnetos that are now being produced in very large quantities by

the three manufacturers specially mentioned. I am naturally in a position to deal more fully, and at greater length, with the products of the B.T.H. Co., but I wish to make it clear that my object in doing this is chiefly to develop a method of analysing the characteristics of a H.T. magneto, which is quite general in its application, and at the same time to indicate certain methods of testing which can be applied to different magnetos to enable these characteristics to be compared.

### (2) M.L. Magneto Syndicate Type A.D.S. Magneto.

This magneto, which is of the rotating armature single-cylinder type intended for use on rotary engines of the Gnome, Mono-soupape, Clerget, or similar types, is based on the original Bosch D.A.L. design, but has been improved in many respects. It is illustrated in Figs. 10 and 11, Fig. 9 showing a sectional view.

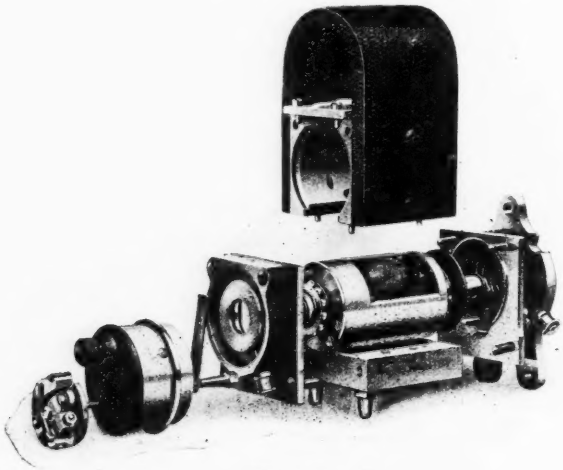


FIG. 11.

Its weight has been reduced by carefully designing the castings without in any way reducing its strength. The fixing of the terminal has been improved, the old socket fitting half in gun-metal and half in aluminium being done away with, and the terminal now screwing directly into the gun-metal end. In addition, the cable connection has been made waterproof and more secure, so as to diminish any risk of the cable becoming detached while running.

The chief modifications, however, which have been found necessary have been in connection with the contact breaker and the cams, as these machines have to run at a very high speed, from 2,500 to 3,500 r.p.m. Consequently, the old Bosch design of flat cam which gave rapid acceleration of the contact breaker arm has been given up, and the cams now fitted are made to a correctly-shaped profile which is ground out on machines specially designed and constructed for the purpose.

In addition, it has been found necessary to increase the length of opening the primary circuit—that is, to reduce the ratio $\frac{\theta_1}{\theta_2} = k$—in order to give time for the spark to die out when the machine is working at a high speed. This alteration has completely removed a trouble experienced with the D.A.L. type of misfiring if the engine were inclined to be oily, and has removed the necessity, previously apparent, for very close and careful setting of the plug points. It

has also increased the life of the contact breaker arm and the platinum points, and has made the machine suitable for use at much higher speeds. The cam, it should be mentioned, is made from a continuous ring of steel, very carefully ground inside and out in order to ensure absolute concentricity.

**Thomson-Bennett Type A.D.4 Magneto.**

This is a four-cylinder machine of the rotating armature type, corresponding in size to the Bosch type Z.U.4 magneto used extensively in this country during the years preceding the outbreak of war. Fig. 12* gives a sectional view of the machine showing the detailed construction, whilst the general appearance can be gathered from Fig. 13.

The Bosch design has been simplified, chiefly in respect of the construction of the bearing for half-speed wheel, but many of the other features have been retained. Very large numbers of these magnetos have been supplied for aeroplanes, and the extended experience gained with them has been entirely satisfactory in every respect.

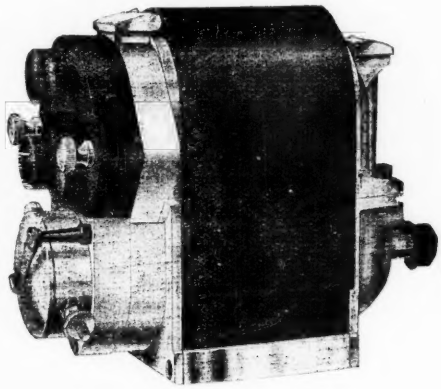


FIG. 13.

**B.T.H. Eight-Cylinder Type A.8.S. Sleeve Inductor Magneto.**

**(1) General Description.**

This magneto is of the sleeve inductor type with fixed armature. It is designed to give four sparks per revolution, and is therefore fundamentally different from the ordinary rotating armature type of magneto, which cannot give more than two sparks per revolution. The sleeve inductor rotates, of course, at engine speed. A six-cylinder machine (Type A.6.S.) is also built on the same principle, designed to run at three-quarter engine speed, but as this differs from the eight-cylinder machine only in respect of the distributor and gearing, I shall confine my attention to the type A.8.S. machine.

Two photographs of the machine are reproduced in Figs. 14 and 15, from which the construction of the various component parts, such as rotating sleeve, fixed armature, distributor, condenser, end plates, and brush holder, clearly shown in the photographs, can be better appreciated. Finally, that vital part of any magneto, namely, the contact breaker, is depicted in detail in Fig. 16. It will be noted that a pivoted steel bell crank lever arm carries at one end a fibre

* Fig. 12 has been omitted.

block which is actuated by the rotating eight part hardened steel cam rigidly fixed to the distributor brush spindle, while at its other end the movable platinum contact screw is secured. A very strong steel spring fixed at one end to the contact lever arm and at the other end to the base plate serves to force the movable contact tightly against the adjustable fixed contact when the cam is not in engagement with the fibre heel.

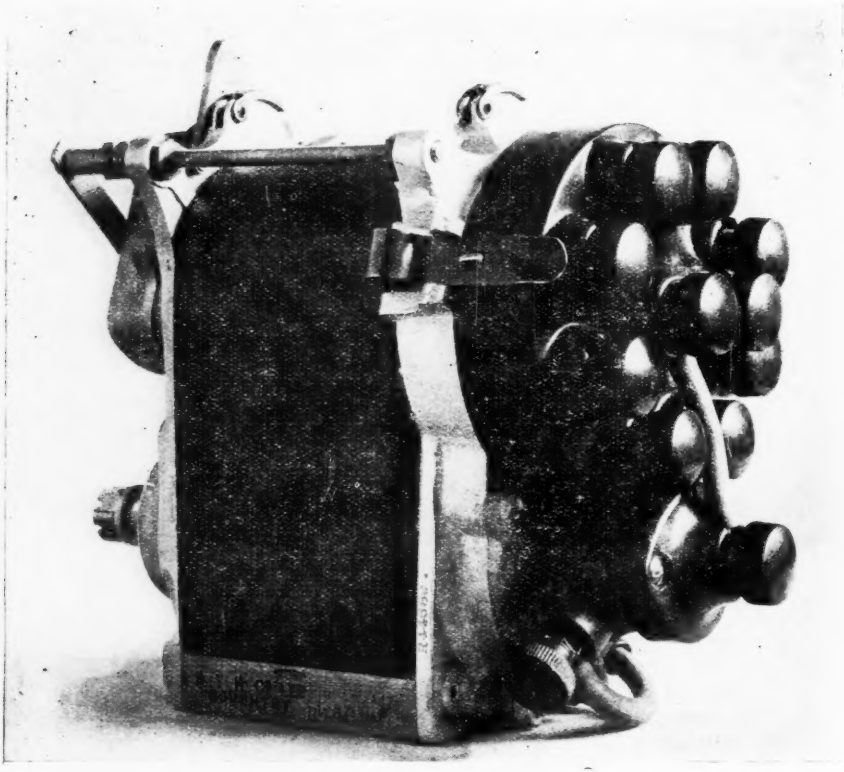


FIG. 14.

It is clear, therefore, that the contacts are opened eight times during each revolution of the cam shaft, which rotates at half the speed of the sleeve inductor, thus giving four breaks per revolution of the latter—as desired.

It is evident from the illustrations and drawings that the manufacture of such a magneto on a large scale must have been attended with considerable difficulties, because there are no less than 397 parts of different design, and a total of 860 parts are actually used in the construction of one machine. Bearing all this in mind, it is very gratifying to know that magnetos of this type are being produced in very large quantities, and the known excellence of these machines speaks volumes for the organisation responsible for their production, which is presided over by Mr. R. Dumas, whose untiring efforts in connection with this work cannot be too highly praised.

## (2) Analysis of Operation Characteristics.

### (a) *Action of the Rotating Sleeve Inductor.*

As with any form of magneto, it is first necessary to analyse the flux changes produced in the armature core during one revolution of the rotating part before the operation characteristics can be determined. Let us, therefore, see what happens when the rotating sleeve is turned through one revolution. Fig. 17 gives eight diagrams corresponding to eight different sleeve positions 45° apart, and on each diagram the approximate flux distribution between the magnet poles is shown. Considering positions 1, 3, 5, and 7, it will be at once noted that in each case the flux through the armature core is zero. Furthermore, for the intermediate positions 2, 4, 6, and 8, the direction of the flux through the core is alternatively positive and negative; that is, for position No. 2 the flux flows upwards through the core, while for position No. 4 it is downwards.

In other words, the flux through the armature core reverses its direction *four* times during each revolution of the sleeve, and by virtue of this fact it is possible to obtain four sparks per revolution. A magneto of this type is sometimes referred to as a *two-cycle* machine, because the flux wave, when plotted (as in Fig. 18) for one revolution of the rotating part, can be split up into

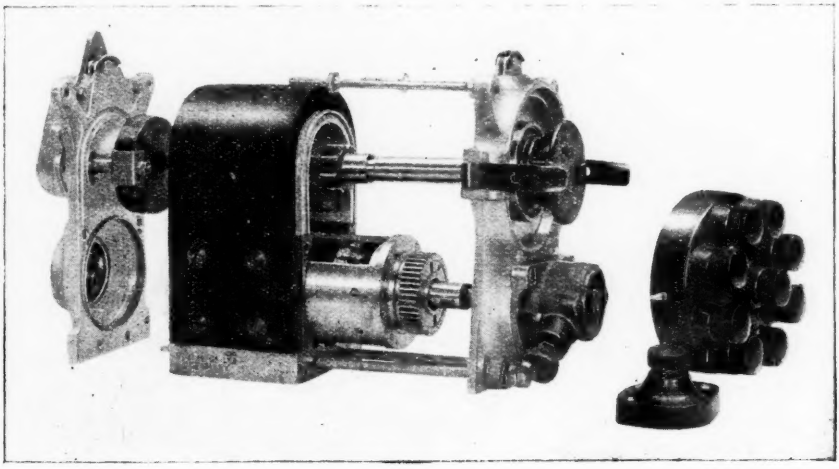


FIG. 15.

two equal parts, each comprising a positive and negative portion. The four points (90° apart) where the flux wave crosses the zero lines correspond to the sleeve positions depicted in positions 1, 3, 5, and 7 (Fig. 17).

The maximum value reached by the flux wave is approximately 32,000 lines. This is the actual flux which links itself with the primary and secondary windings carried by the fixed armature core. Assuming a leakage coefficient of 1.4, the magnet flux works out at 45,000 lines.

The following figures, which are of interest, have been calculated :—

Flux density in magnets ... ... = 5,900 lines per sq. cm.
,, ,, ,, armature core ... = 8,500 ,, ,,

These figures are very high for an inductor type of magneto, although they are lower than the figures worked to in a magneto with rotating armature. The

difference is accounted for by the fact that in the type "A" magneto—as in all inductor type machines—there are four air gaps in that portion of the magnetic circuit lying between the pole faces, as compared with only two gaps in the

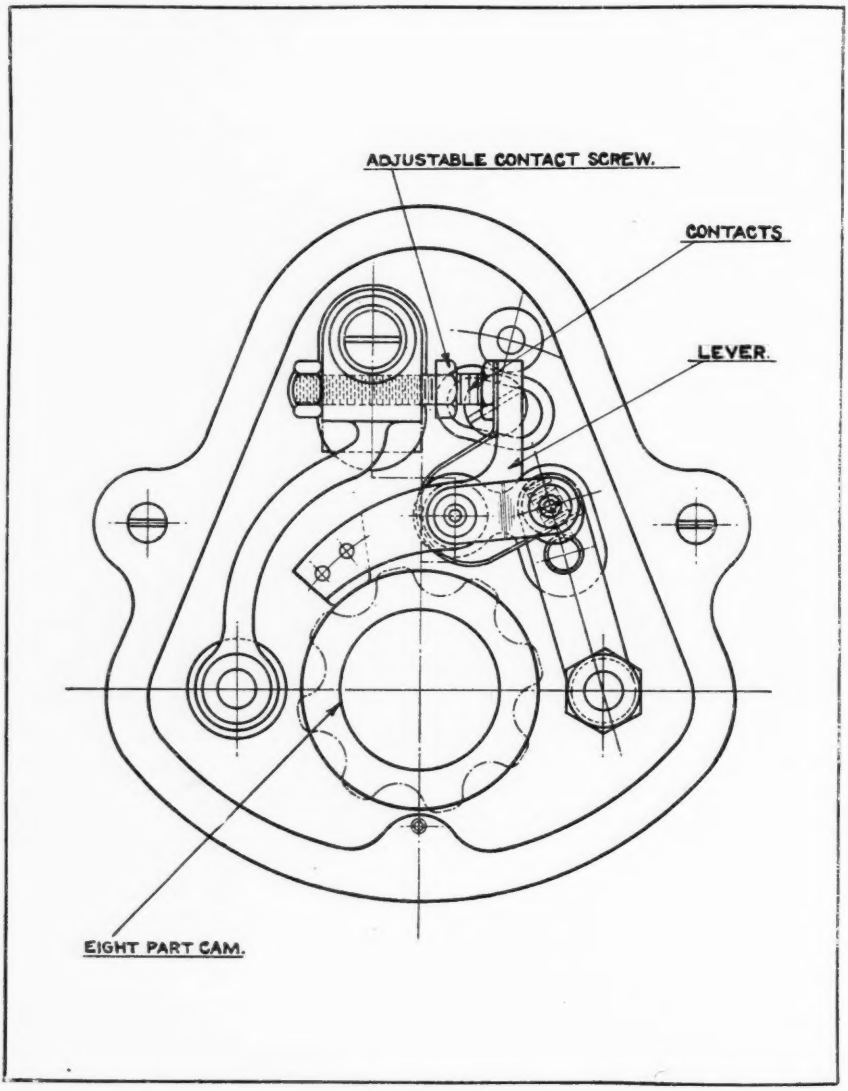



FIG. 16.

two-spark rotating armature type of magneto. The increased reluctance due to the larger number of air gaps naturally means that the available flux is smaller, using magnets of corresponding size and strength.

(*b*) *Induced Voltage due to Rotation.*

Having determined the flux wave, it is an easy matter to compute the induced voltage wave resulting simply from the rotation of the sleeve, it being assumed

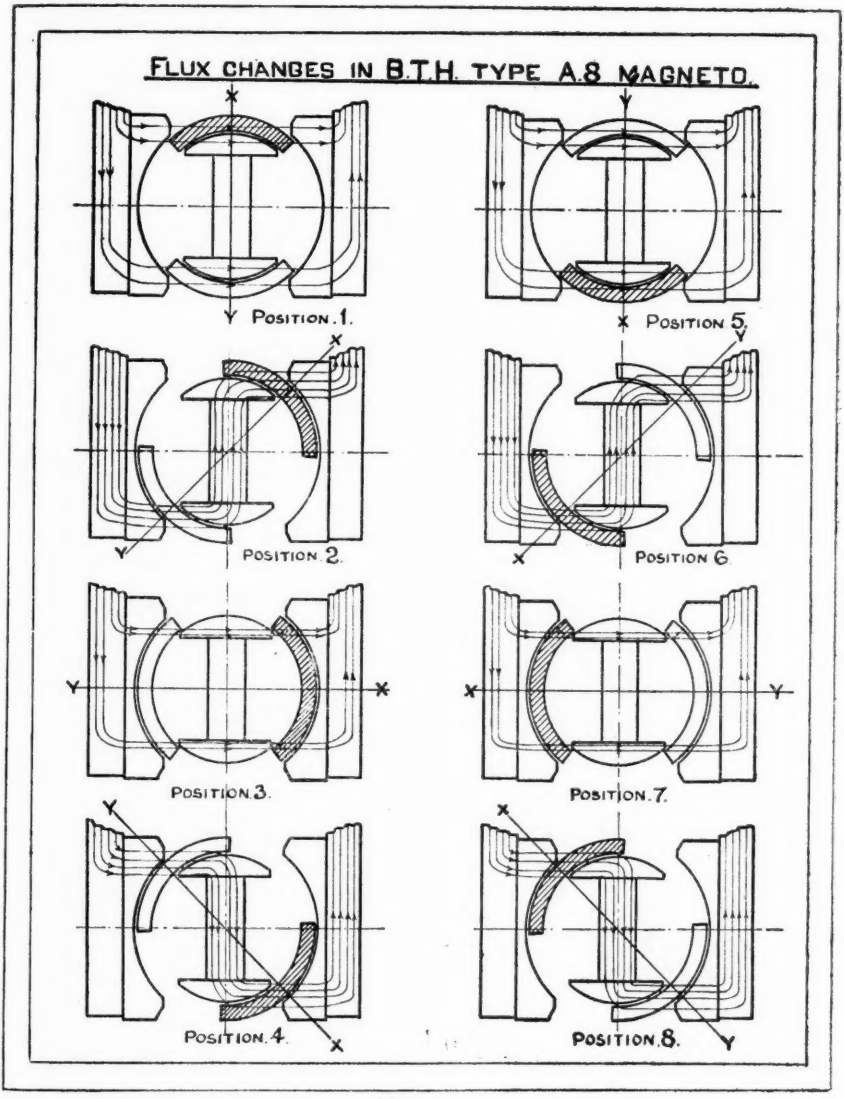



FIG. 17.

that the contact breaker is inoperative and that the primary is open-circuited continually. *Vice-versa,* the flux wave can be calculated from the voltage wave, and in Fig. 18 this has actually been done, the voltage wave being a reproduction of an actual oscillogram giving the induced primary voltage at 1,000 r.p.m.

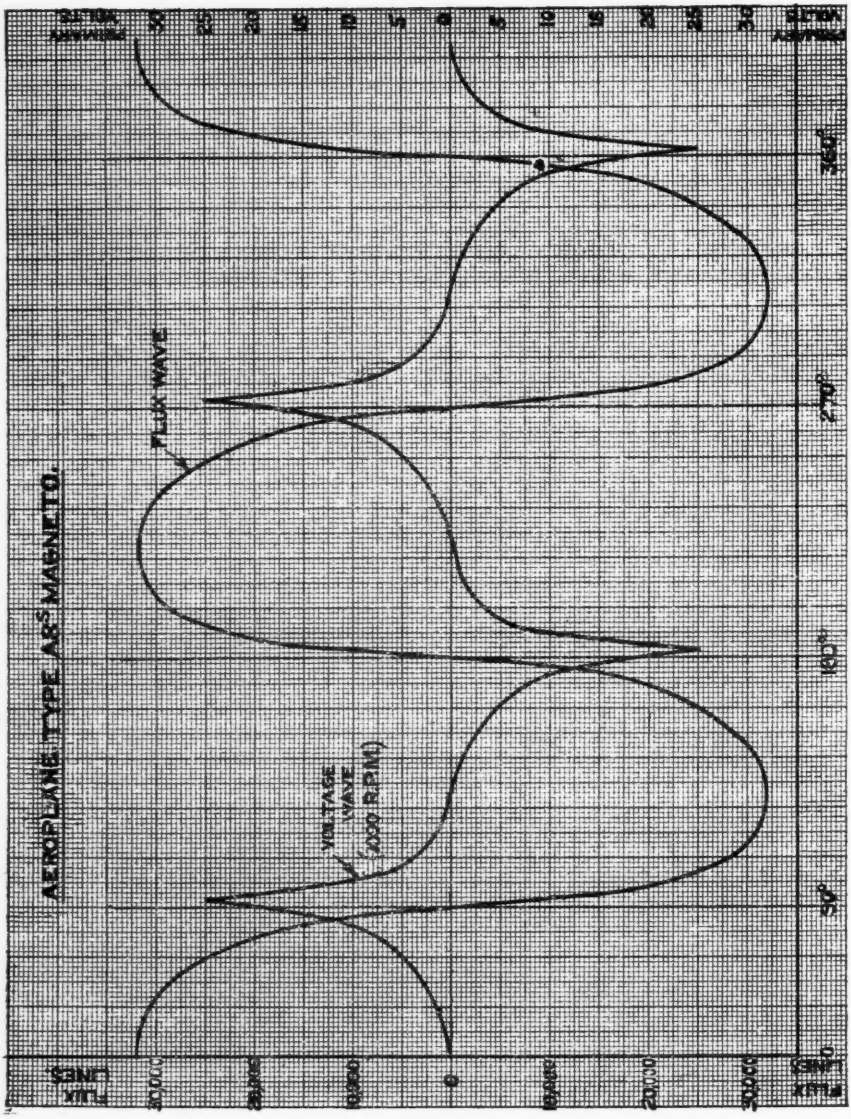

AEROPLANE TYPE A8 MAGNETO.
FLUX WAVE
VOLTAGE WAVE (4000 R.P.M.)
FLUX LINES
ELEC. ANGLE
0°
90°
180°
270°
360°

FIG. 18.

Considering the secondary, a voltage is induced by the rotation of the sleeve which conforms in shape with the primary voltage wave. The maximum point reached by the wave will, however, be $k_1$ times the maximum primary voltage. The ratio $k_1$ for this particular magneto is very large, so that, considering the machine as a simple alternator, the maximum secondary voltage will be in the neighbourhood of 3,000 at very high speeds.

We have assumed that when the axis of the rotating sleeve is exactly at right angles to the magnetic axis, the flux is zero—corresponding to position 1, in Fig. 17, and position 0° in Fig. 18. This means that the induced voltage due to rotation reaches its maximum value just after the sleeve has passed through this critical position, which I shall call the "dead centre." Actually, the forward phase displacement of this maximum point amounts to 3°.

(*c*) *Primary Current at "Break"* (*Ib*).

Adopting the graphical method of calculating the primary current curve during the period that the contacts are closed, previously described, I have been able to

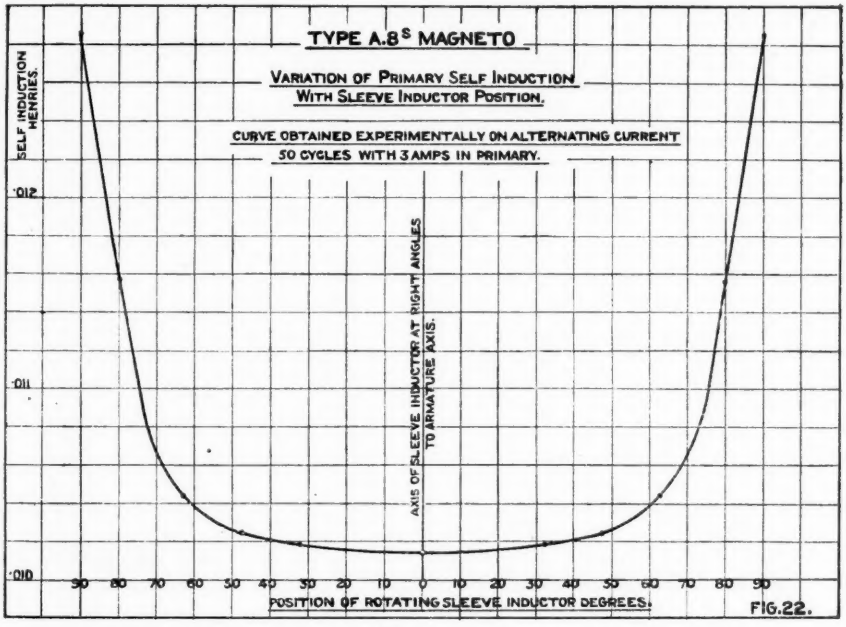

TYPE A.8S MAGNETO
VARIATION OF PRIMARY SELF INDUCTION WITH SLEEVE INDUCTOR POSITION.
CURVE OBTAINED EXPERIMENTALLY ON ALTERNATING CURRENT 50 CYCLES WITH 3 AMPS IN PRIMARY.
SELF INDUCTION HENRIES.
·012
·011
·010
AXIS OF SLEEVE INDUCTOR AT RIGHT ANGLES TO ARMATURE AXIS.
90 80 70 60 50 40 30 20 10 0 10 20 30 40 50 60 70 80 90
POSITION OF ROTATING SLEEVE INDUCTOR DEGREES.
FIG.22.

FIG. 19.

plot a number of curves for six different speeds ranging between 100 r.p.m. and 3,000 r.p.m. The curves are shown in Figs. 20 and 21, the basis of the calculation being the open circuit primary voltage curve as determined by an actual oscillograph test, reproduced in Fig. 18, and the primary self-induction curve given in Fig. 19.

These curves show that the maximum value of the primary current increases with the speed; very rapidly at low speeds, but hardly at all when the speed is high. In fact, at abnormally high speeds the maximum value will actually fall off owing to the damping caused by the eddy currents induced in the rotating iron segments, which has the effect of preventing the flux in the stationary armature

FIG. 18.

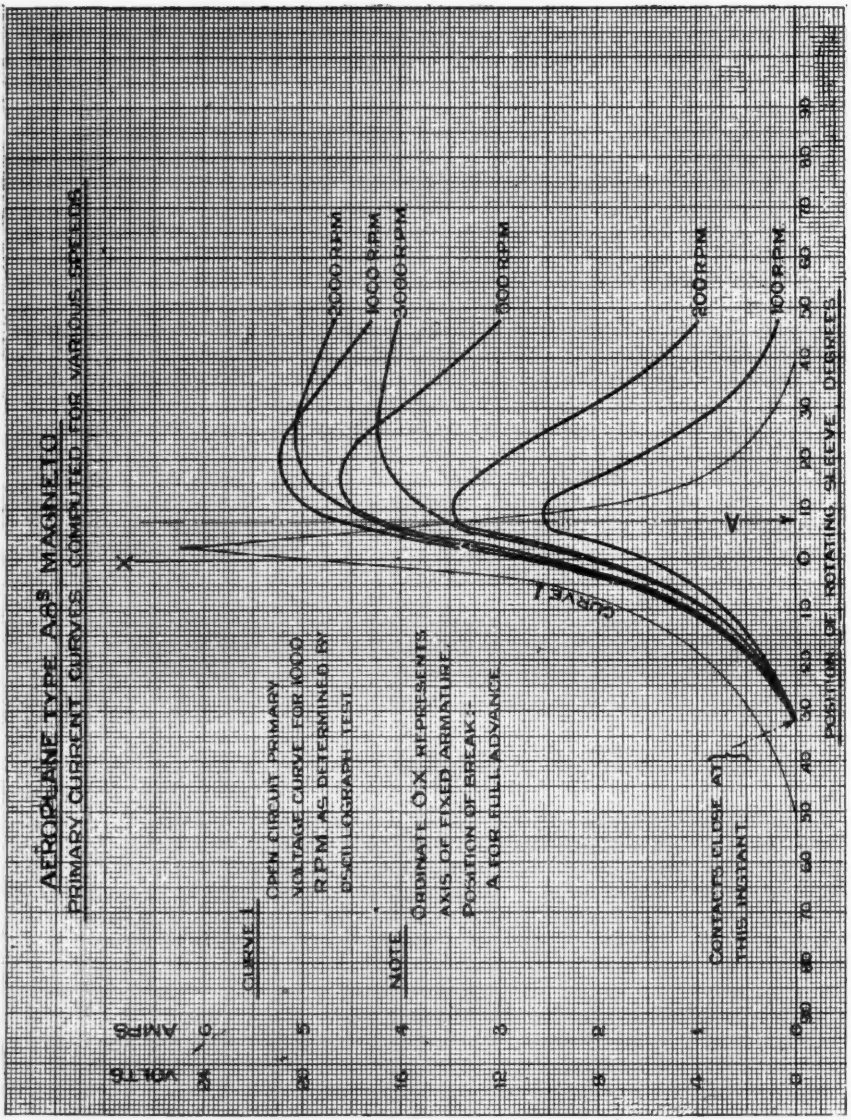

AEROPLANE TYPE ARS MAGNETO
PRIMARY CURRENT CURVES COMPUTED FOR VARIOUS SPEEDS
VOLTS
AMPS
CURVE 1
OPEN CIRCUIT PRIMARY VOLTAGE CURVE FOR 1000 R.P.M. AS DETERMINED BY OSCILLOGRAPH TEST
NOTE
ORDINATE O X REPRESENTS AXIS OF FIXED ARMATURE
POSITION OF BREAK - A FOR FULL ADVANCE
CONTACTS CLOSE AT THIS INSTANT
2000 R.P.M.
1000 R.P.M.
900 R.P.M.
800 R.P.M.
400 R.P.M.
100 R.P.M.
CURVE 1
POSITION OF ROTATING SLEEVE, DEGREES

FIG. 20.

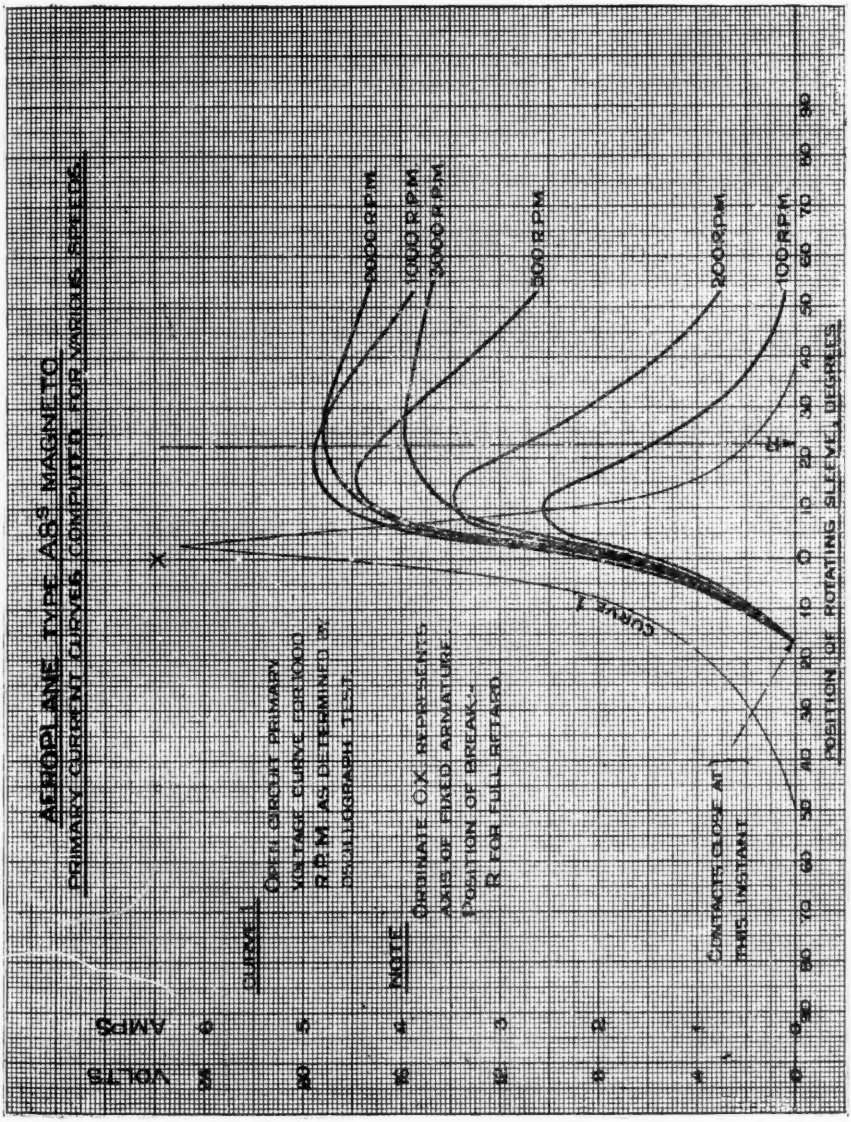

AEROPLANE TYPE A83 MAGNETO
PRIMARY CURRENT CURVES COMPUTED FOR VARIOUS SPEEDS
CURVE I
OPEN CIRCUIT PRIMARY VOLTAGE CURVE FOR 1000 R.P.M. AS DETERMINED BY OSCILLOGRAPH TEST.
NOTE
ORDINATE OX REPRESENTS AXIS OF FIXED ARMATURE.
POSITION OF BREAK R FOR FULL RETARD
CONTACTS CLOSE AT THIS INSTANT
2000 R.P.M.
1000 R.P.M.
3000 R.P.M.
500 R.P.M.
200 R.P.M.
100 R.P.M.
CURVE I
VOLTS
AMPS
POSITION OF ROTATING SLEEVE, DEGREES

FIG. 21.

core rising to its proper maximum value after each reversal. It is important to note also that the maximum point on the current curve is, as it were, displaced forward in the direction of motion as the speed is increased.

It has been necessary to plot two sets of primary current curves, one (Fig. 20) corresponding to the fully-advanced position of the timing lever and the other set (Fig. 21) corresponding to the fully-retarded position. Actually, there is very little difference between the two curves corresponding to any particular speed, although when the timing lever is fully retarded it will be noted that a slightly lower maximum value is reached simply because the contacts closed at a little later period, and the early portion of the voltage wave is therefore not included in the integration. In Fig. 22 I have plotted four curves showing the variation in the maximum primary current with the speed for fully-advanced and retarded positions of the timing lever respectively, and correspondingly the variation in the displacement of this maximum point for the same positions of the timing lever.

In Fig. 20 I have indicated, by means of a chain dotted line, the position of the rotating sleeve at "break," so that by determining where this line intersects these six curves it is an easy matter to arrive at the value of $I_B$ for each speed corresponding to the fully-advanced position of the timing lever. Similarly, the position of "break" for the fully-retarded position is indicated in Fig. 21, and I give below in the form of a table the values deduced from these two sets of curves for the six speeds under consideration :—

| | ADVANCE. | | | | RETARD. | | | |
|---|---|---|---|---|---|---|---|---|
| Speed R.P.M. | Maximum current I amps. | Phase displacement $\omega$ | $I_B$ amps. | Ratio $\frac{I_B}{I}$ | Maximum current I amps. | Phase displacement $\omega$ | $I_B$ amps. | Ratio $\frac{I_B}{I}$ |
| 100 | 2.55 | 8° | 2.54 | .996 | 2.57 | 9.5° | 1.52 | .591 |
| 200 | 3.45 | 11° | 3.41 | .988 | 3.47 | 12° | 2.8 | .807 |
| 500 | 4.62 | 17° | 4.26 | .922 | 4.47 | 16.5° | 4.3 | .962 |
| 1,000 | 5.23 | 20° | 4.55 | .870 | 4.88 | 20° | 4.87 | .998 |
| 2,000 | 5.08 | 25° | 4.24 | .835 | 4.77 | 25° | 4.76 | .998 |
| 3,000 | 4.22 | 27° | 3.5 | .829 | 3.97 | 27° | 3.94 | .992 |

*(d) Sparking Voltage and Energy Output.*

The maximum voltage will be approximately proportional to the value of $I_B$, so that if we plot this factor against the speed we shall obtain a curve that is substantially representative of the way in which voltage increases with the speed. This is done in Fig. 23, a second curve being added to represent the variation in the maximum available energy with the speed, as calculated from the formula :—

$$\text{Maximum available energy} = \tfrac{1}{2}LI_B^2.$$

The whole of the maximum available energy does not appear in the high-tension spark, as already explained, owing to certain losses.

**(3) Results of Tests.**

*(a) Voltage and Energy Curves.*

Maximum sparking voltage and spark energy curves obtained by experiment are plotted in Fig. 24.

Experiment has shown that under normal working conditions a new sparking plug requires about 3,500 volts to cause a spark to jump between the electrodes when the piston is in the firing position and the compression is about 80lbs. per square inch. It is therefore interesting to note that this voltage is given by the magneto at the very low speed of 60 r.p.m., the point being indicated on the

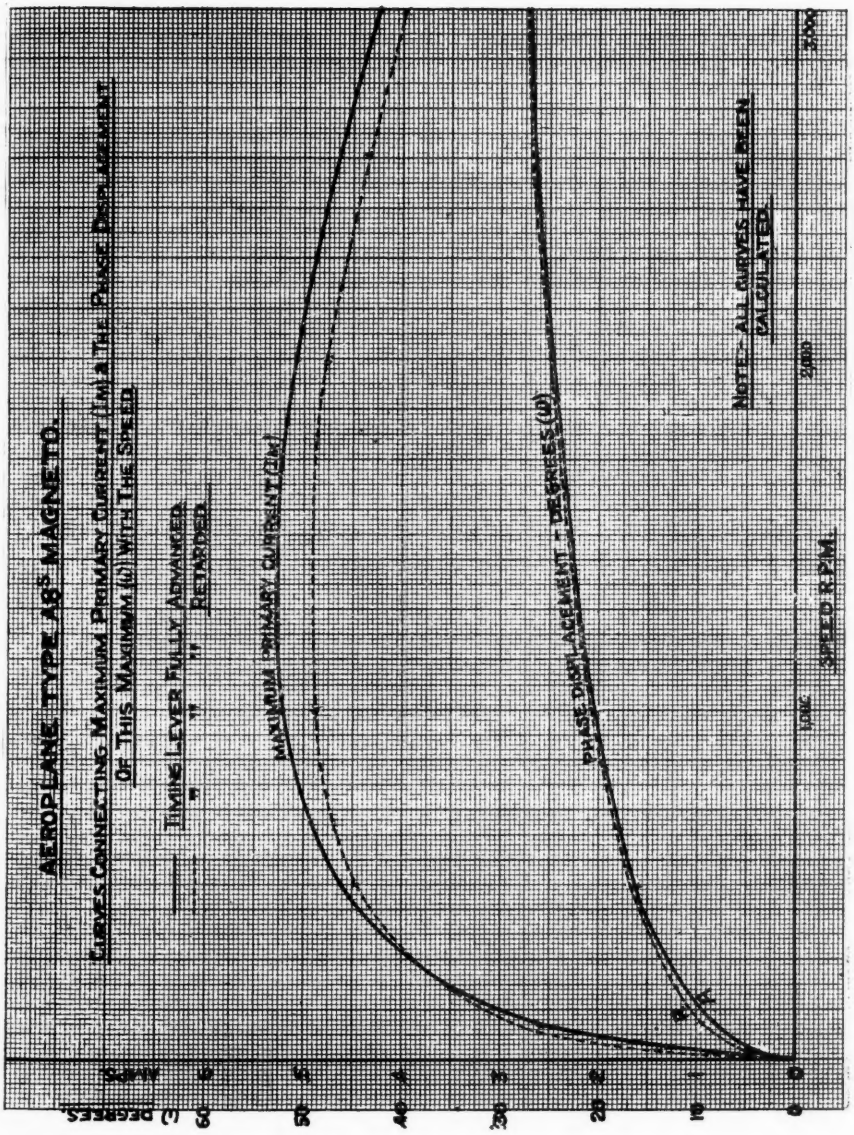

AEROPLANE TYPE A8 MAGNETO.
CURVES CONNECTING MAXIMUM PRIMARY CURRENT (Im) & THE PHASE DISPLACEMENT OF THIS MAXIMUM (ϕ) WITH THE SPEED
TIMING LEVER FULLY ADVANCED
" RETARDED
MAXIMUM PRIMARY CURRENT (Im)
PHASE DISPLACEMENT - DEGREES (ϕ)
NOTE:- ALL CURVES HAVE BEEN CALCULATED.
SPEED R.P.M.
1000
2000
3000
ϕ DEGREES
Im AMPS.

FIG. 22.

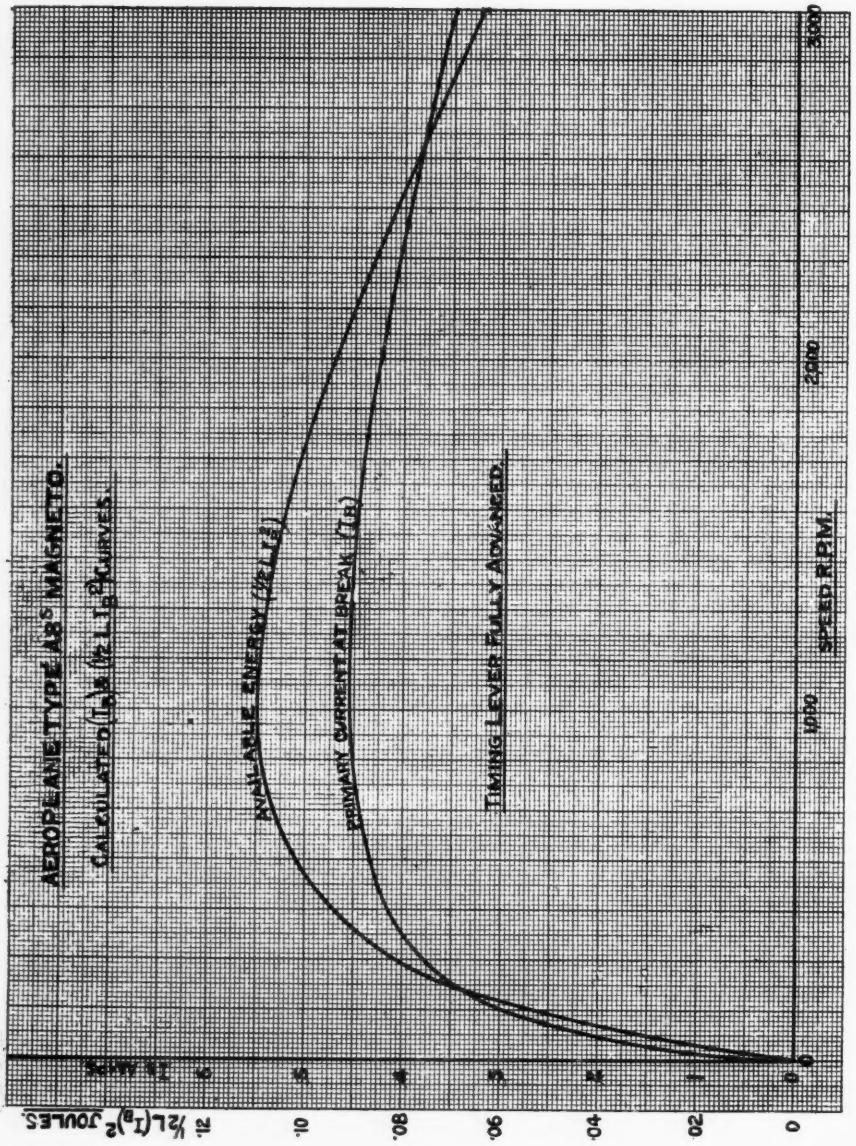

AEROPLANE TYPE A.B.S MAGNETO.
CALCULATED $(T_B)^2$ & $(\frac{1}{2}LI_B^2)$ CURVES.
AVAILABLE ENERGY $(\frac{1}{2}LI_B^2)$
PRIMARY CURRENT AT BREAK $(T_B)$
TIMING LEVER FULLY ADVANCED.
$\frac{1}{2}L(T_B)^2$ JOULES.
SPEED R.P.M.
0
1000
2000
3000
.02
.04
.06
.08
.10
.12

FIG. 23.

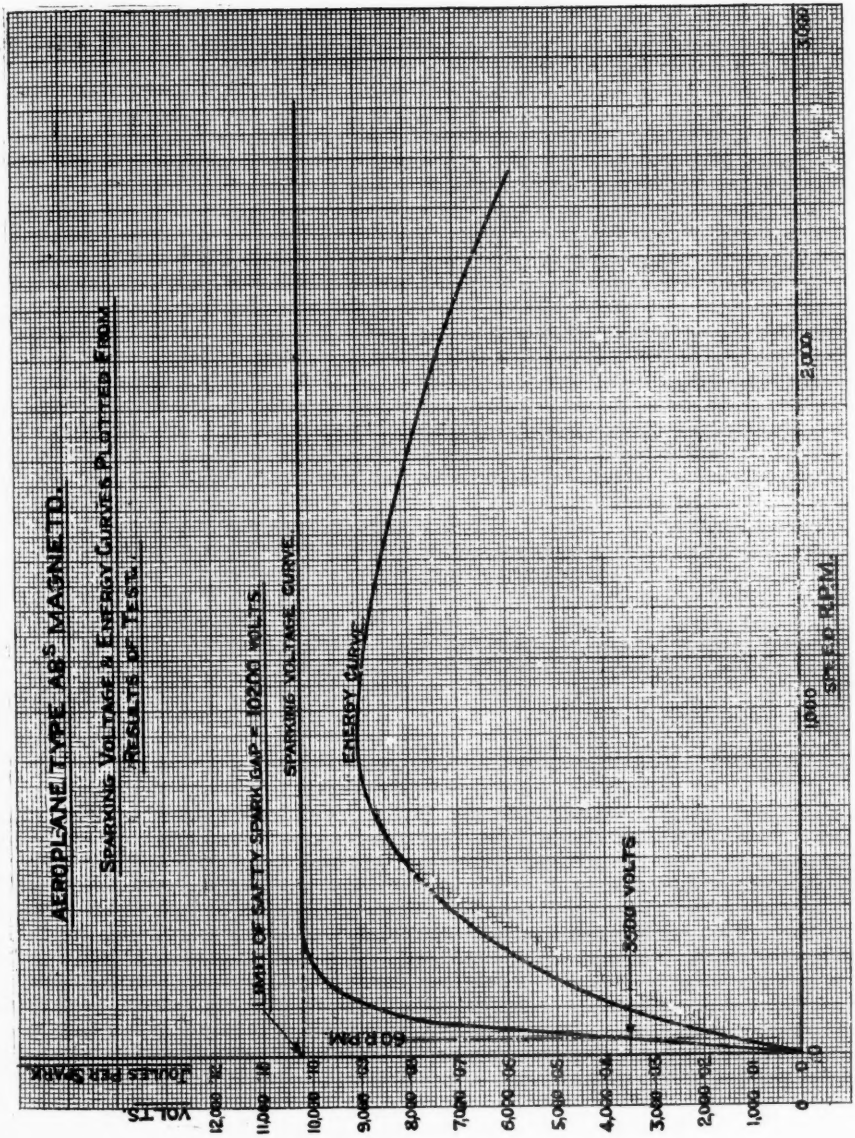

AEROPLANE TYPE A.B.S. MAGNETO.
SPARKING VOLTAGE & ENERGY CURVES PLOTTED FROM RESULTS OF TEST.
LIMIT OF SAFETY SPARK GAP = .0200 INCHES.
SPARKING VOLTAGE CURVE
ENERGY CURVE
6000 R.P.M.
5000 VOLTS
VOLTS
JOULES PER SPARK
SPEED R.P.M.
0
1,000
2,000
3,000

FIG. 24.

curve. The normal running speed is 1,800 r.p.m. One would naturally expect to find that a magneto having these characteristics would give satisfactory ignition at the moment of starting, and this is actually borne out in practice.

The energy output per spark at each speed was determined by a calorimetric method, the test extending over an appreciable time. The curve plotted from these results conforms very closely in shape with the maximum available energy curve ($\frac{1}{2}LI_B^2$) given in Fig. 23, although the measured energy for each speed is about 80% of the calculated maximum value. The remaining 20% of the energy is dissipated in the iron masses in the magnetic circuit and in the windings.

*(b) Oscillograph Curves.*

In Figs. 25, 26, and 27, a number of oscillograms for a magneto of this type are reproduced, and these show that the actual value of $I_B$ agrees substantially with the calculated value in every case.

*(c) Variation of Armature Core Flux with Speed.*

By integrating the open circuit primary voltage half wave for each speed it is an easy matter to determine the corresponding values of the armature core flux ($N$).

Another method of determining the value of the armature core flux at different speeds, which has the advantage of simplicity, is to connect a Kenotron valve between the insulated end of the primary and earth, in series with a very large non-inductive resistance and a micro-ammeter of the moving-coil type. The arrangement of connections for a test of this kind is shown in Fig. 28.

The combination is calibrated by determining the D.C. voltage necessary to produce the deflection observed for any given speed, the other conditions of the circuit remaining unaltered. In this way the unknown resistance of the valve itself is allowed for.

In general, if :—

$v$ = D.C. voltage as determined by calibration test.
$n$ = Speed r.p.m.
$a$ = No. of sparks per revolution.
$S_1$ = Primary turns.
$N$ = Armature core flux (maximum).

Then :—

$$N = 6 \times 10^9 . \frac{v}{n\ a\ S_1} \text{ lines.}$$

It is not surprising to find that the value of $N$ gradually diminishes as the speed is increased. This falling off of the flux $N$ results from the eddy currents set up in the rotating and stationary iron masses in the magnetic circuit, which have the effect of damping back the flux at high speeds. This explains why it is that the energy curve begins to drop at about 1,000 r.p.m. ; if eddy currents could be entirely eliminated, there would be no such drop in the curve, and the flux $N$ would remain practically the same at all speeds.

It is highly desirable, therefore, to arrange the magnetic circuit so that the deleterious effect of eddy currents is reduced to a minimum. This can be more readily done in a polar inductor type of magneto if careful attention is given to certain details of the design, and the 12-cylinder magneto of this type to be described later is superior to the sleeve inductor type in this respect.

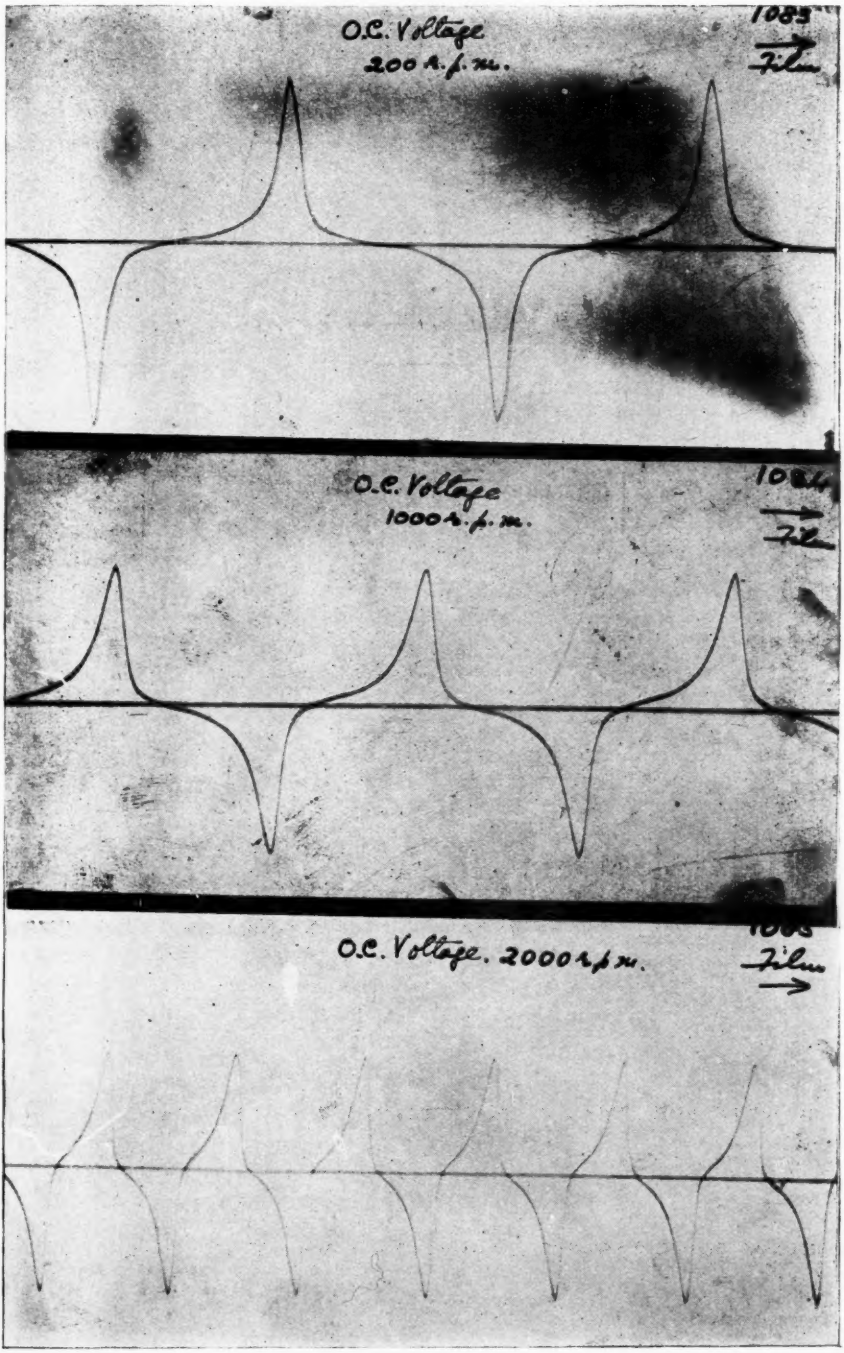

O.C. Voltage
200 r.p.m.
Film
O.C. Voltage
1000 r.p.m.
O.C. Voltage. 2000 r.p.m.
Film

FIG. 25.

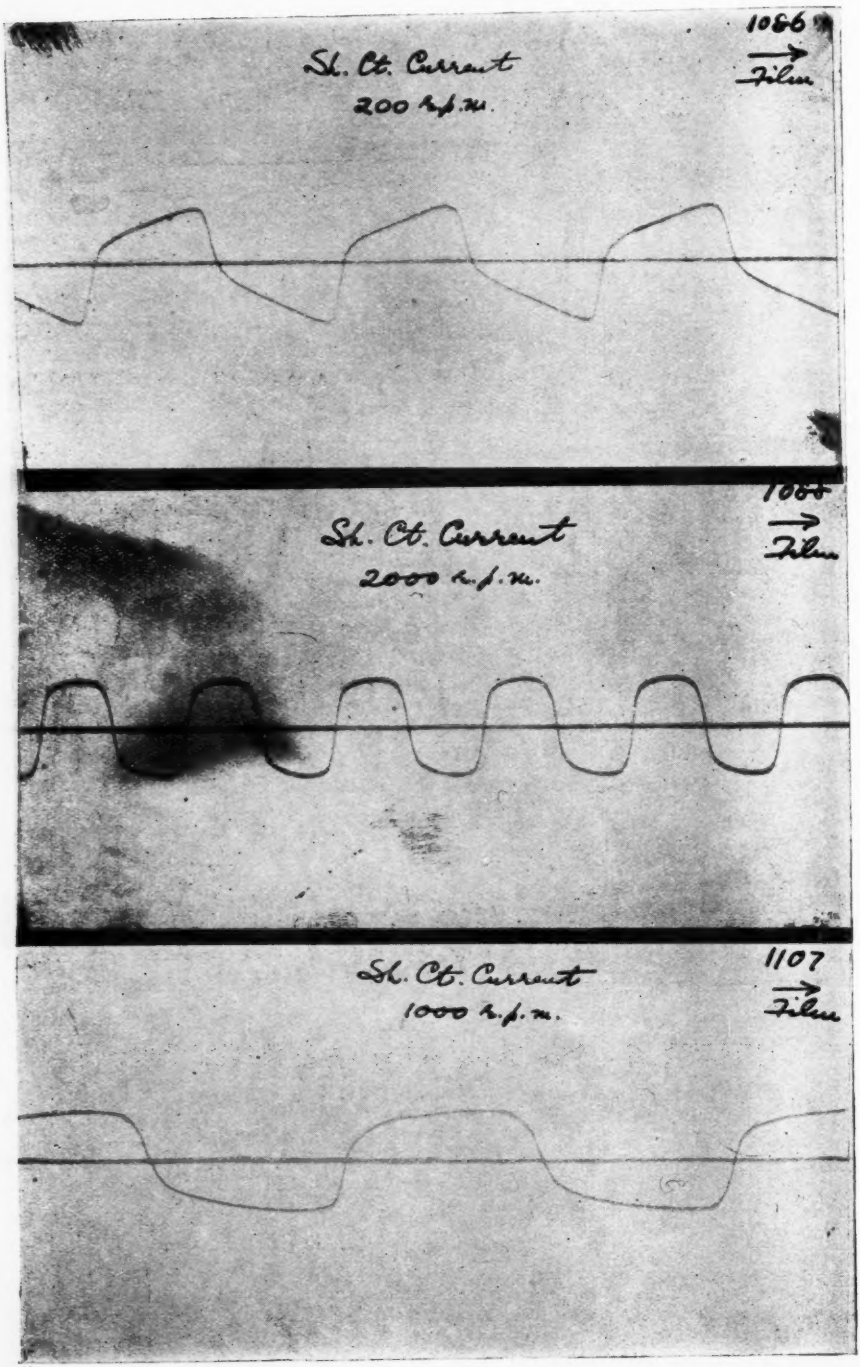

1086
Sh. Ct. Current
200 r.p.m.
Film
1087
Sh. Ct. Current
2000 r.p.m.
Film
1107
Sh. Ct. Current
1000 r.p.m.
Film

FIG. 26.

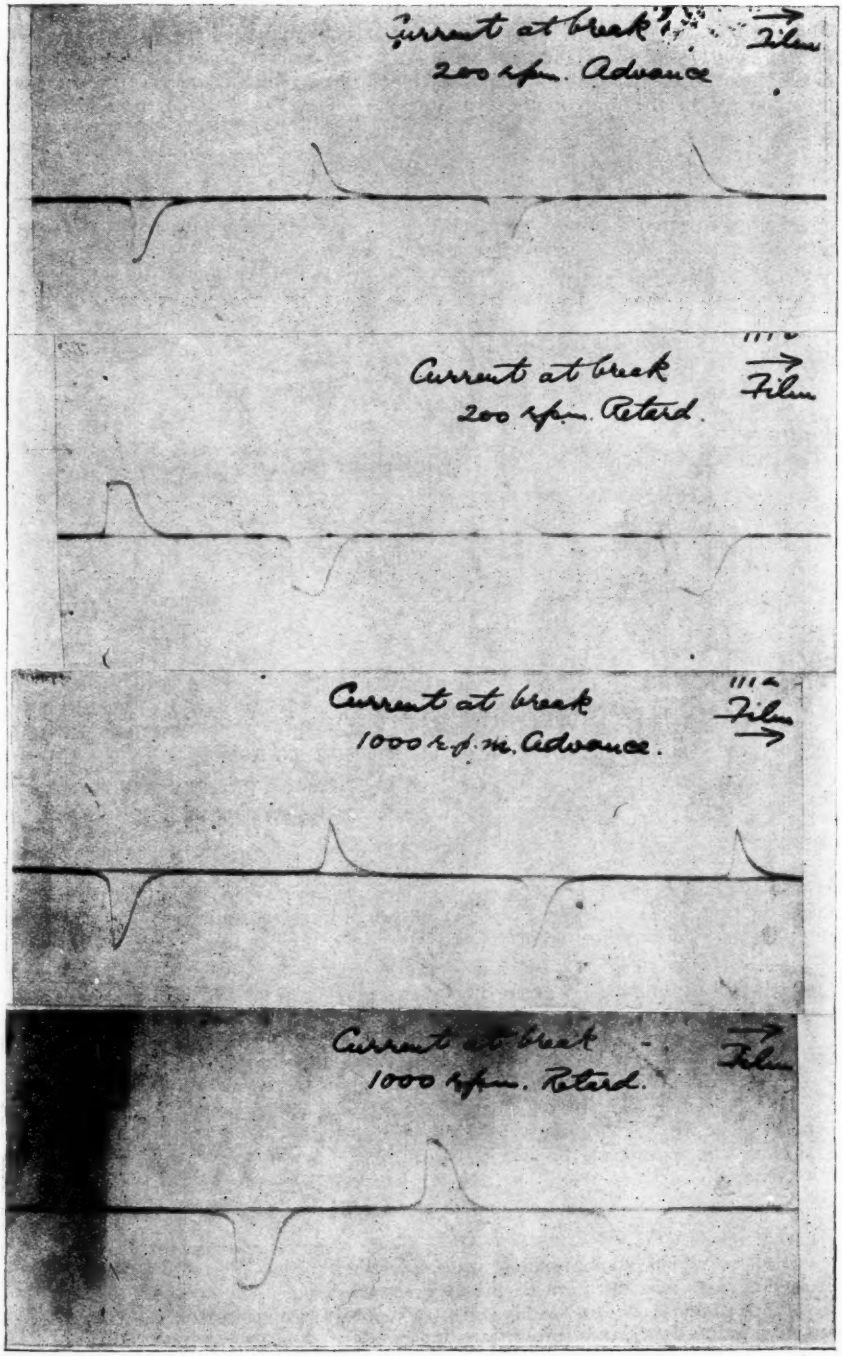

Current at break
200 r.p.m. Advance
Film
Current at break
200 r.p.m. Retard.
Film
Current at break
1000 r.p.m. Advance.
Film
Current at break
1000 r.p.m. Retard.
Film

FIG. 27.

The rotating armature type of magneto as standardised by Bosch is a very bad offender from this standpoint, and oscillograph tests recently carried out on one of the Bosch Company's latest machines—built since the war—showed that at 4,000 r.p.m. the drop in the armature core flux $N$ amounted to as much as 23%.

*(d) Primary Current and Voltage Curves.*

Curve two in Fig. 29 shows the relationship between the R.M.S. value of the open circuit primary voltage (measured by a voltmeter) and the speed. Curve one shows the variation in the short circuit primary current, as measured by an ammeter. The shape of the voltage curve also shows that the armature core

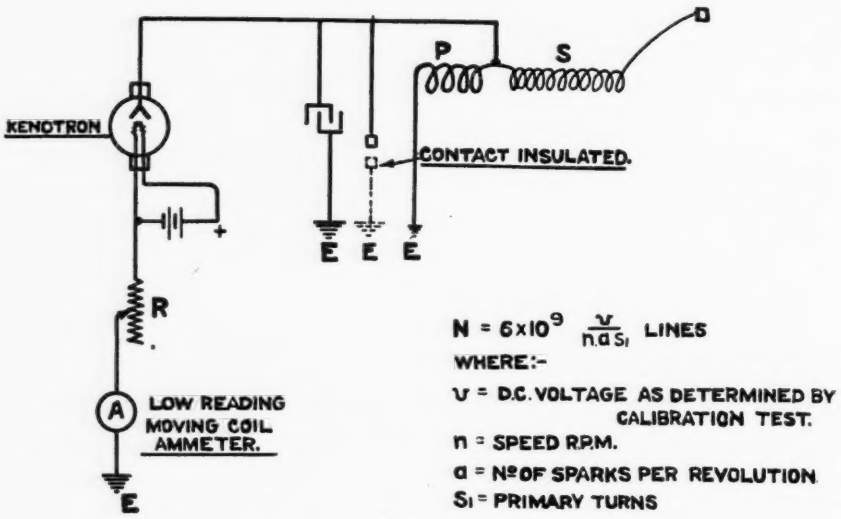



FIG. 28.

flux $N$ diminishes as the speed is increased, because if $N$ remained constant, curve one would be a straight line. The dotted curves 3 and 4 are for a Bosch magneto of corresponding type (*HL8*).

A very good idea of what the operation characteristics of a magneto are likely to be can be gathered from the value of the open circuit primary voltage at some definite speed. It is the practice of the B.T.H. Co. to measure the voltage given by every magneto at 2,000 r.p.m. immediately after magnetising. It must not be less than a certain minimum figure. In addition, the machine is then run at 2,000 r.p.m. with the secondary short-circuited and the voltage again measured. A minimum percentage drop in voltage is allowed, which must not be exceeded. Generally speaking, this percentage drop affords some indication of the magnetic quality of the magnets; the larger the drop the lower the coercive force, and *vice-versa*.

*(e) Circle Diagram.*

Fig. 30 shows some oscillograms which have been plotted in the form of circle diagrams, the duration of the H.T. spark being indicated. A diagram of this kind enables one to readily form a mental picture of the growth of the currents induced in both primary and secondary during one revolution of the

sleeve inductor. It is quite obvious that similar diagrams can be constructed for any type of magneto if sufficient data is available. My object in giving such a diagram for the type "A" magneto is simply to illustrate the method.

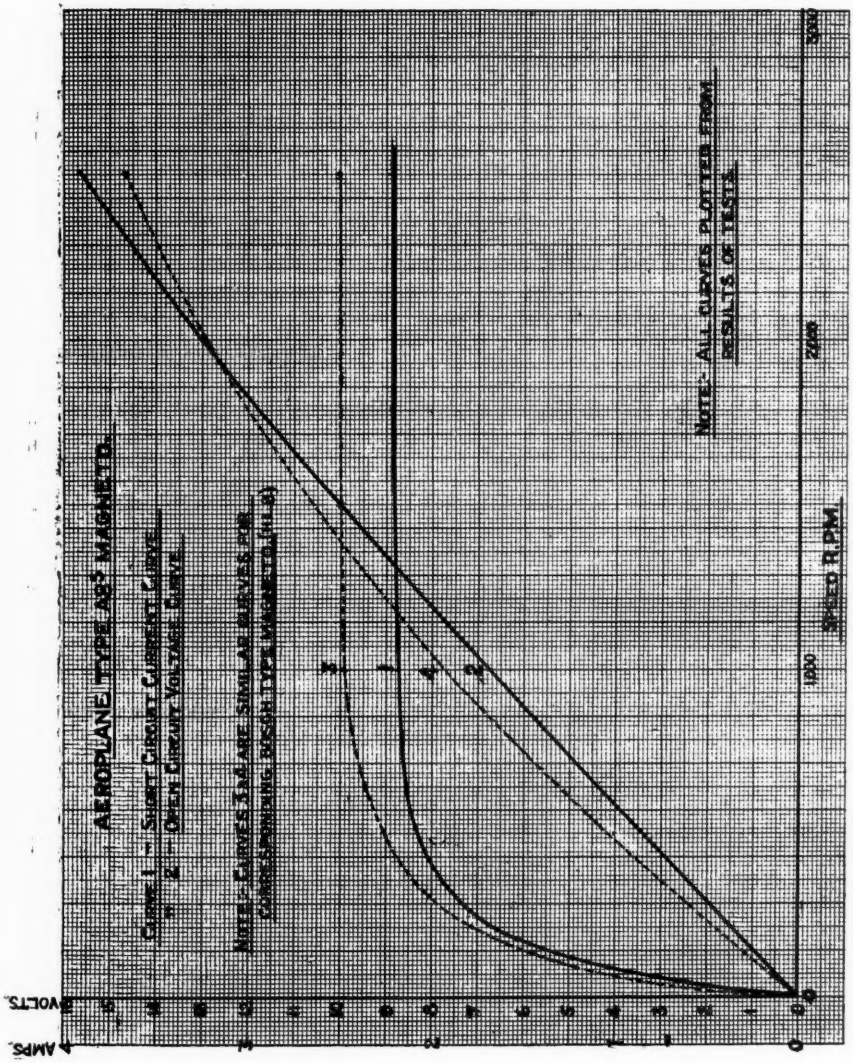

AEROPLANE TYPE A&S MAGNETO.
Curve 1 – Short Circuit Current Curve.
" 2 – Open Circuit Voltage Curve.
Note-Curves 3 & 4 are similar curves for corresponding Rosen type magneto (H.T.)
Note-All curves plotted from results of tests.
VOLTS.
AMPS.
SPEED R.P.M.
1000
2000
3000

FIG. 29.

*(f) Overall Efficiency.*

By measuring the power required to drive a magneto at different speeds, it is an easy matter to calculate, from the spark energy output, the overall efficiency of the machine. Three curves are given in Fig. 31 for the type "A" magneto, curve one showing the variation in the driving power (watts) with the speed; curve two the variation in the output, in watts, taking the energy curve in Fig. 24

as a basis; and curve three the variation in efficiency. The latter curve is calculated from the formula :—Efficiency $= \frac{\text{Output.}}{\text{Input.}}$

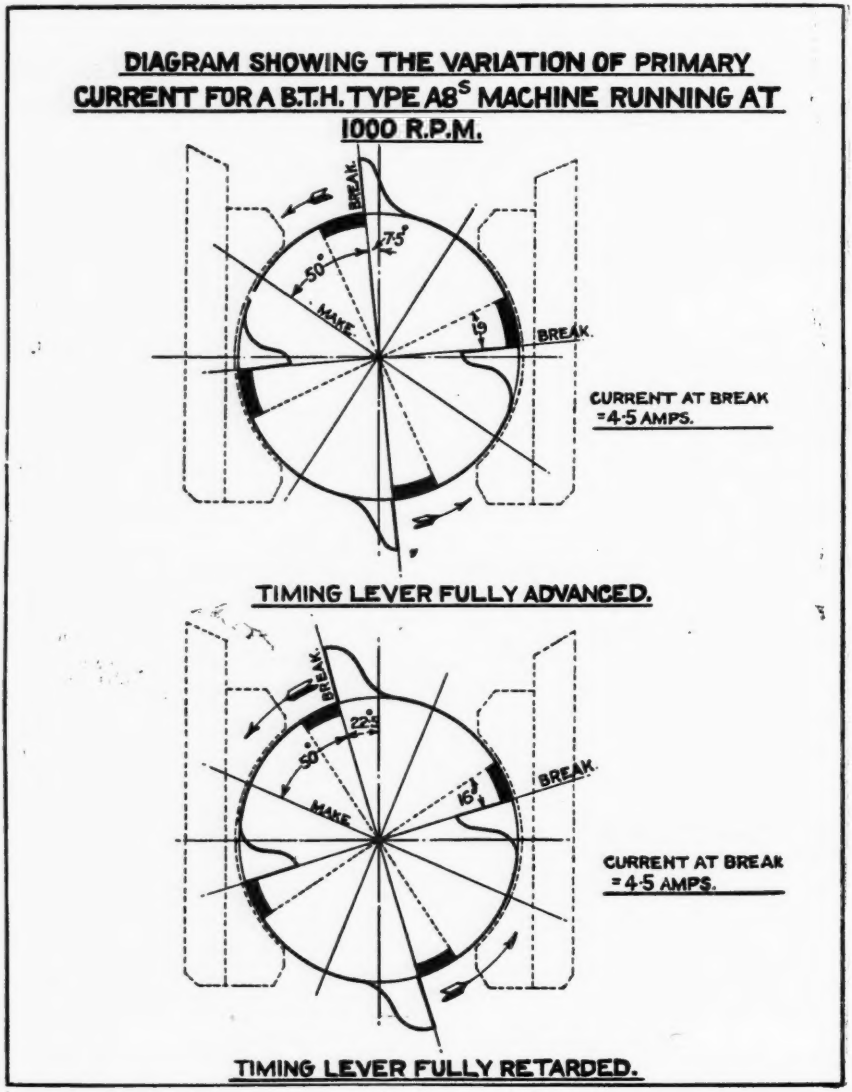



FIG. 30.

The efficiency values are, of course, abnormally low, and chiefly of academic interest, but on the other hand a curve of this kind affords a ready means of comparing the characteristics of different magnetos of the same type.

**(4) The Special "Spark Gap" Distributor.**

When discussing the question of the voltage rise in the secondary at

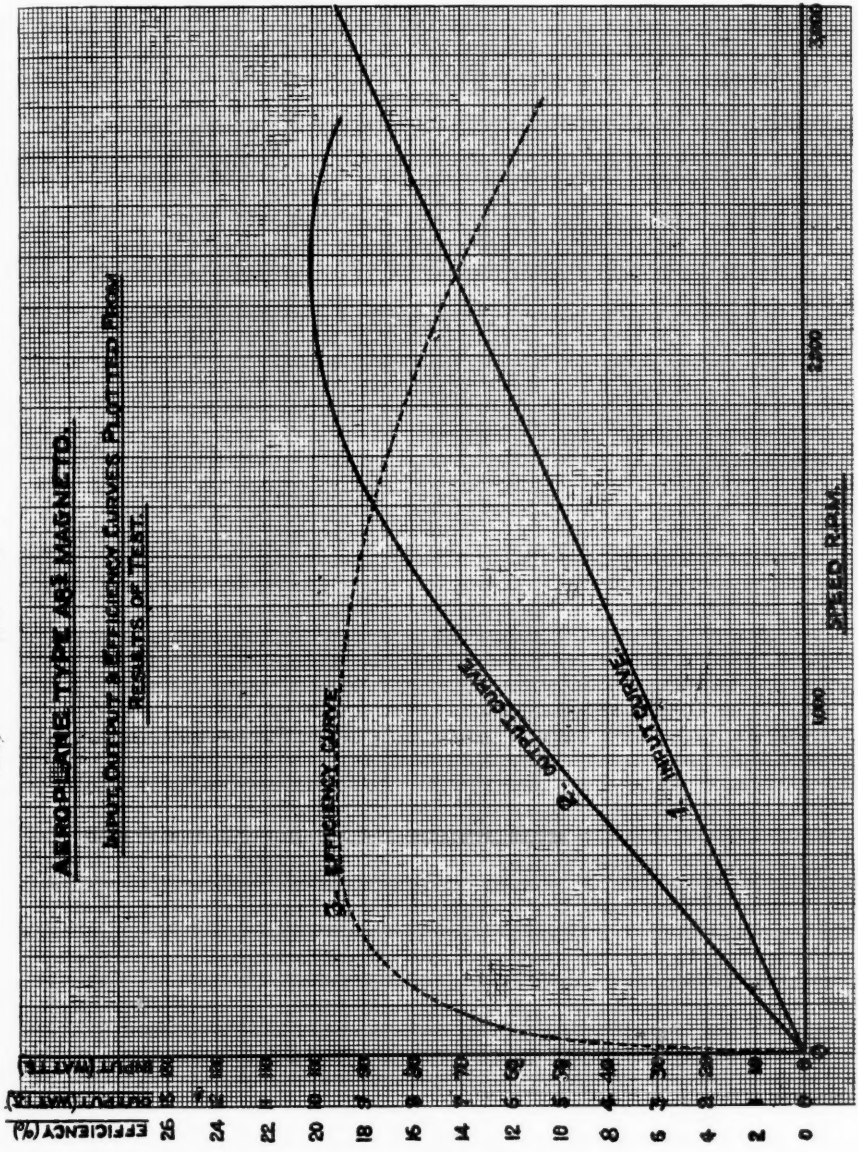
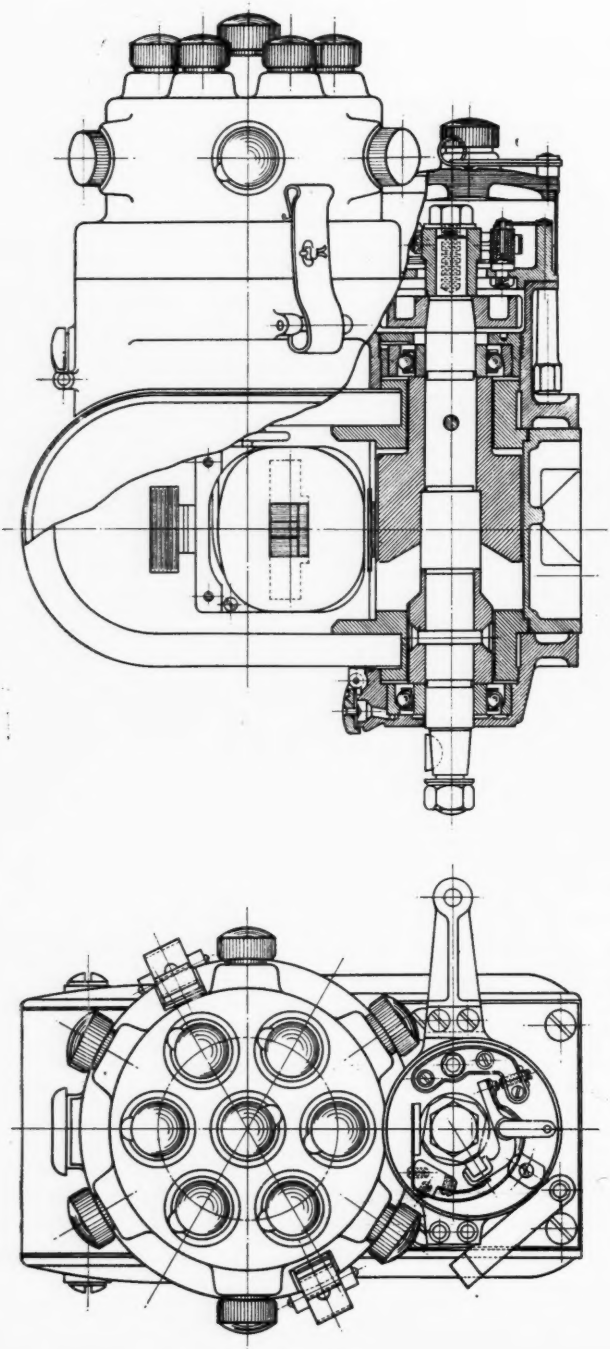



FIG. 31.

"break," I referred to the advantages likely to result from the use of a distributor brush that is designed to allow a very slight clearance between the face of the brush and the distributor segment. Such an arrangement is used in



ASSEMBLY B.T.H.-12 CYLINDER POLAR INDUCTOR TYPE MAGNETO.

FIG. 32.

the type "A" magneto, and it has proved to be thoroughly reliable and satisfactory.

The distributor brush is made of steel, and has V grooves in its active face with a central slot for taking the square head of the locking screw. It is of rectangular section, and it is free to move in a brass tube moulded into the insulation. A spring forces the brush outwards against the head of the locking screw.

Obviously, the screw is automatically locked, and the air gap between the face of the brush and the track of the distributor can be readily adjusted by first depressing the brush until the head of the screw is clear and then turning the latter either one way or the other. A quarter of a turn on the screw, which gives

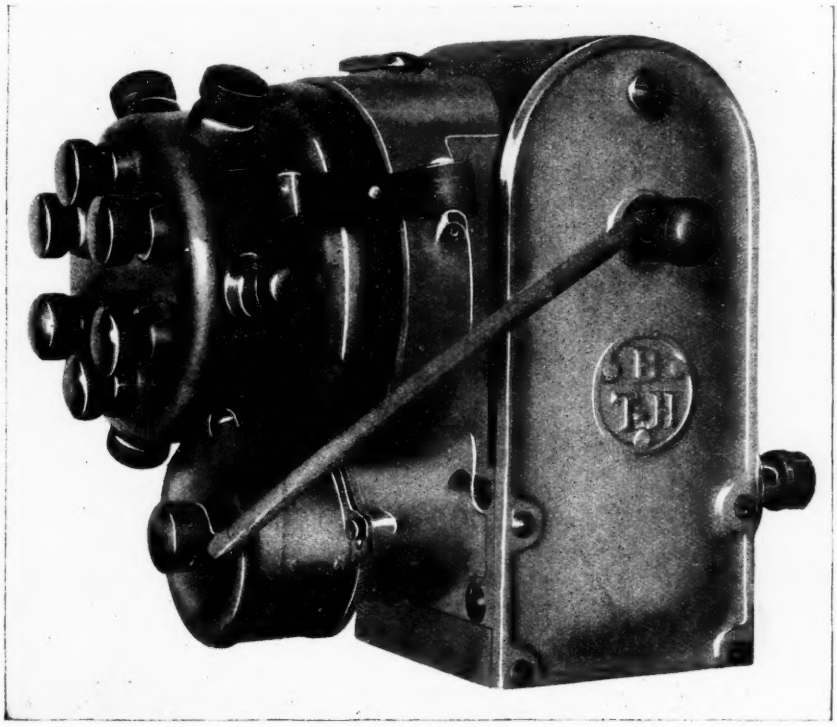


FIG. 33.

the minimum adjustment, alters the air gap by 0.002in. When properly adjusted, the air gap should be between .01in. and .014in.

The amount of wear on the face of the brush after continuous use is so small as to be almost negligible. We have run a brush of this kind on a magneto at 2,500 r.p.m. for 300 hours, and at the end of that time the face of the brush was, for all practical purposes, as good as that of a new brush. There was no sign of any pitting.

**(5) Mechanical Considerations.**

It does not seem to be generally appreciated that the manufacture of magnetos can only be undertaken successfully if very skilled labour is available,

and the necessary steps are taken to machine all the component parts to extremely fine limits. The type "A" magneto is so complicated in design that the difficulties encountered in manufacturing it on a commercial basis are, without doubt, much greater than in the case of any other type of magneto being produced in England at the present time.

Take, for example, the rotating sleeve inductor. It comprises two soft iron segments which are secured by means of screws and dowel pins to a non-magnetic nickel steel end plate and spindle on the one hand, and to a gun-metal end plate carrying the distributor gear-wheel driving pinion on the other. The whole combination has to run on three ball bearings, all of which must be in perfect alignment. The outer race of one bearing is carried by the aluminium driving end plate, whilst the inner races of the other two are securely fixed to the respective ends of the stationary armature end plates.

The clearance on each side of the sleeve inductor is only 0.006in., and it is clear, therefore, that extraordinary precautions must be taken in manufacture to ensure that after assembly the sleeve will be perfectly free and neither too tight nor too loose in its bearings.

In the manufacture of the type "A" magneto no less than 716 limit gauges, made up as follows, are employed :—

| Type of Gauge. | No. of Gauges of Different Design. |
|---|---|
| Plug Gauge ... ... ... ... ... | 51 |
| Ring ,, ... ... ... ... ... | 18 |
| Snap ,, ... ... ... ... ... | 71 |
| Plate Gauges— | |
| Length ... ... ... ... ... | 12 |
| Height ... ... ... ... ... | 3 |
| Depth ... ... ... ... ... | 4 |
| Roller Gauges— | |
| Height ... ... ... ... ... | 7 |
| Depth ... ... ... ... ... | 13 |
| Total = | 179 |

The gauges itemised above constitute one complete set. Four complete sets of gauges are in use, these being allocated as follows :—

(1) For Shop use.
(2) ,, ,, Inspectors.
(3) ,, Government Inspectors.
(4) ,, Check purposes in Tool-room.

The shop gauges are made to somewhat finer limits than are specified on the drawing, so that the rejections made by the Inspection Department are in consequence reduced to an absolute minimum. The finer limit gauges are checked in the Tool-room every day, whilst other gauges are checked periodically, the frequency of the inspection being dependent upon the degree of accuracy worked to and the liability of the gauge to wear in use.

The system of working is as follows :—When the "go" end of a gauge (which is made somewhat larger than the minimum diameter specified) has worn down to the drawing dimension, the gauge is passed on to the Inspectors for use in inspecting the work. This arrangement ensures that there is always an ample supply of very accurate gauges in the Inspection Department because the wear on gauges used in the shops is much more rapid than the wear from use by the Inspectors. Furthermore, each gauge has, by this method of working, a

more extended life. Gauges are withdrawn from the Inspection Department for very slight errors from drawing dimensions, and the cost of maintaining a large number of gauges within narrow limits of accuracy is naturally quite a considerable item.

The finest limits worked to are for the hole in the contact breaker cam and the end of the distributor brush spindle, on which the cam is fitted. These limits are:—

| | | | |
|---|---|---|---|
| Hole in Cam ... | ... | Maximum diameter | 13.005 mm. |
| | | Minimum ,, | 12.995 ,, |
| | | Tolerance | 0.01 ,, |
| End of Spindle | ... | Maximum diameter | 12.995 ,, |
| | | Minimum ,, | 12.988 ,, |
| | | Tolerance | 0.007 ,, |

I refer specifically to this one feature of the design so as to dissipate any false notion that the manufacture of magnetos is a relatively easy problem, which may possibly exist in the minds of the uninitiated.

## B.T.H. 12-Cylinder Polar Inductor Magneto.*

The B.T.H. Co. has recently developed a polar inductor type magneto, more especially for 12-cylinder working, although it is proposed to standardise an eight-cylinder machine of exactly the same design by simply changing the distributing mechanism and gearing. I believe I am right in saying that this is the first 12-cylinder magneto to be developed and standardised in this country. Several models have already been constructed and tested with entirely satisfactory results on engines in the test shop, as well as in the air. It is designed to give four sparks per revolution, and the 12-cylinder model runs at 1-1/2 times engine speed.

This magneto is inherently of much simpler construction than the sleeve inductor machine already described, because the sleeve inductor, which by virtue of its design is a very difficult component part to manufacture, is replaced by a polar inductor so designed that it can be easily made, and which at the same time is a more rigid and reliable mechanical structure. Other features in the design combine to greatly simplify the manufacturing problems, and it is anticipated that, other things being equal, the output of magnetos will rise very considerably after the change over from one type to the other has been completely effected—a fact of vital importance at the present critical juncture.

### (1) Description of the Magneto.

A sectional view of the machine is given in Fig. 32. The polar inductor consists of a "straight through" shaft made of non-magnetic nickel steel, on to which two polar inductors are first pressed up against a shoulder, and then finally riveted in position to the shaft. The ends of the magnets are secured to two soft iron pole pieces, near the top of each by means of screws. There is an extremely fine air gap between the inside face of each pole piece and the outside surface of the annular portion of the inductor which it surrounds.

The magnet flux, therefore, passes through this fine annular air gap in a

* The basic principle embodied in this design of producing the requisite flux reversals in a stationary laminated armature circuit by means of a rotating polar inductor co-operating with fixed permanent magnets, is a British invention made by Mr. T. B. Murray, of the Albion Motor Car Company, and first disclosed by him in patent specification No. 14737, dated June 28th, 1906. This patent is now void.

radial direction, and flows from one inductor to the other through the laminated armature circuit. The armature windings are carried by a brass spool which is secured to a laminated iron core, which in turn is fixed by means of two clamping screws to two upright laminated projections. These are fixed in the central casting which carries the pole pieces and also provides the base of the machine.

The contact breaker is operated by a four-part cam fixed to the end of the driving shaft. The contact breaker lever is specially designed so as to give a very low moment of inertia, and tests have shown that it will function satisfactorily

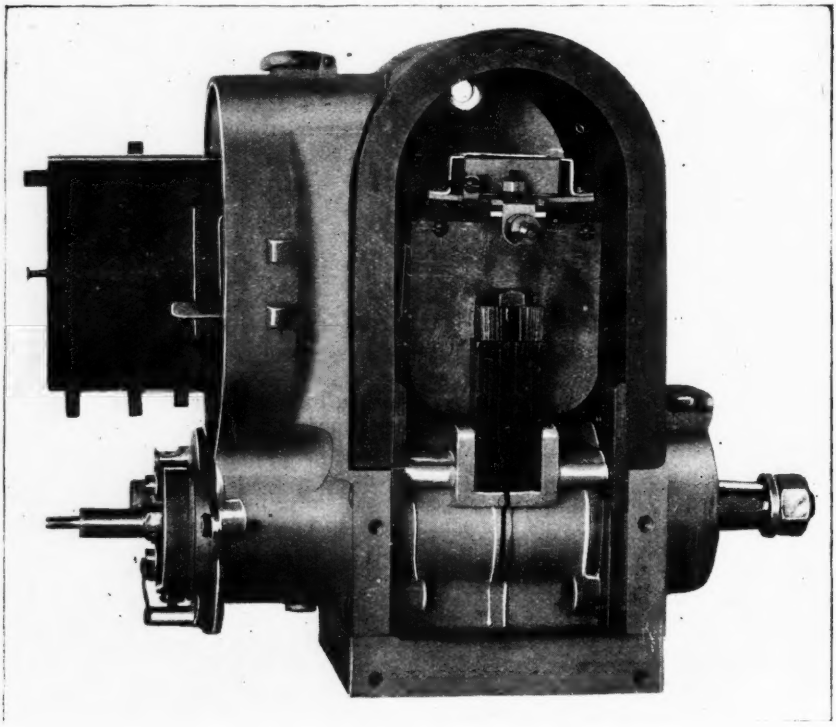


FIG. 34.

when making 16,000 breaks per minute. This frequency corresponds to a speed of 4,000 r.p.m.

The distributor is of special design, having three distinct tracks, two of which are the distributing tracks, each provided with six segments. The brush holder contains five carbon brushes, and the safety gap is incorporated in the brush holder and rotates with it. A gauze window is provided in the front of the distributor, and in consequence of the rotation of the brush holder any products of ionisation have an easy chance of escape. This arrangement is much to be preferred to a totally enclosed stationary type of safety gap, where the silent brush discharge is always liable to produce injurious results.

A good idea of the general appearance of this magneto can be formed by referring to Fig. 33, which gives an outside view of the machine, while the component parts are clearly depicted in Fig. 34.

### (2) Sparking Characteristics and Energy Output.

The magneto possesses excellent sparking characteristics at low speeds, as shown by the curve in Fig. 35, which has been plotted from the results of an actual test. The energy output, although not abnormally high, is very satisfactory for an inductor type magneto. The curve plotted from the figures obtained by a calorimeter test is also given in Fig. 35.

It should be noted that the maximum energy output which is reached at a speed of about 800 r.p.m. is as large as .072 joules.

Special precautions have been taken in the design of the magnetic circuit as well as in the choice of suitable magnetic material to ensure that the effect of eddy current damping is reduced to an absolute minimum. As a result of this, the falling off in the energy output as the speed is increased beyond the maximum point on the curve is not very marked, and for the sake of comparison another curve (shown dotted), which has been plotted from the figures obtained on testing a 12-cylinder polar inductor type magneto of American manufacture, is added. In this case the effect of eddy current damping at high speeds is very pronounced indeed, and the energy output, which reaches a maximum of .065 joules at about 800 r.p.m., falls away to only 0.15 joules at 3,000 r.p.m. There is no doubt that by paying more careful attention to the design of the magnetic circuit as referred to above the characteristic of this particular magneto, so far as energy output is concerned, could be improved.

### (3) Primary Voltage and Current Curves.

Fig. 36 shows two curves, one giving the variation in the open circuit primary voltage (R.M.S. value) with the speed and the other the variation in the short circuit primary current (R.M.S. value) corresponding to the same range of speed. The open circuit voltage curve is practically a straight line passing through the origin, which confirms the view that the eddy current damping in the magnetic circuit is extremely small. With most types of magnetos the open circuit primary voltage curve shows a distinct drop with increase in speed, due to the fact that the active armature core flux diminishes as the speed is raised. This drop is fairly pronounced in the case of the sleeve inductor type magneto already described, as shown by the open circuit voltage curve given in Fig. 29.

The short circuit current, on the other hand, remains substantially constant for all speeds above about 600 r.p.m., and this test, in which the magneto is run with the primary short-circuited through an ammeter of negligible resistance, is analogous to the case of an alternator running on short circuit. Every electrical engineer knows that in the latter case the short circuit current is practically independent of the speed, due to the fact that the induced E.M.F. in the short-circuited winding varies in almost strict proportion to the reactance of the winding.

### (4) Primary Voltage Kick at "Break."

The primary voltage kick at the instant of "break" was determined at different speeds by using a Kenotron in conformity with the method of testing shown in Fig. 5 and previously referred to. The voltage increased with the speed up to 3,000 r.p.m., very rapidly at first, but not so rapidly at very high speeds. Abnormally high voltages were measured, these being considerably in excess of any figures that I have been able to obtain by testing in the same

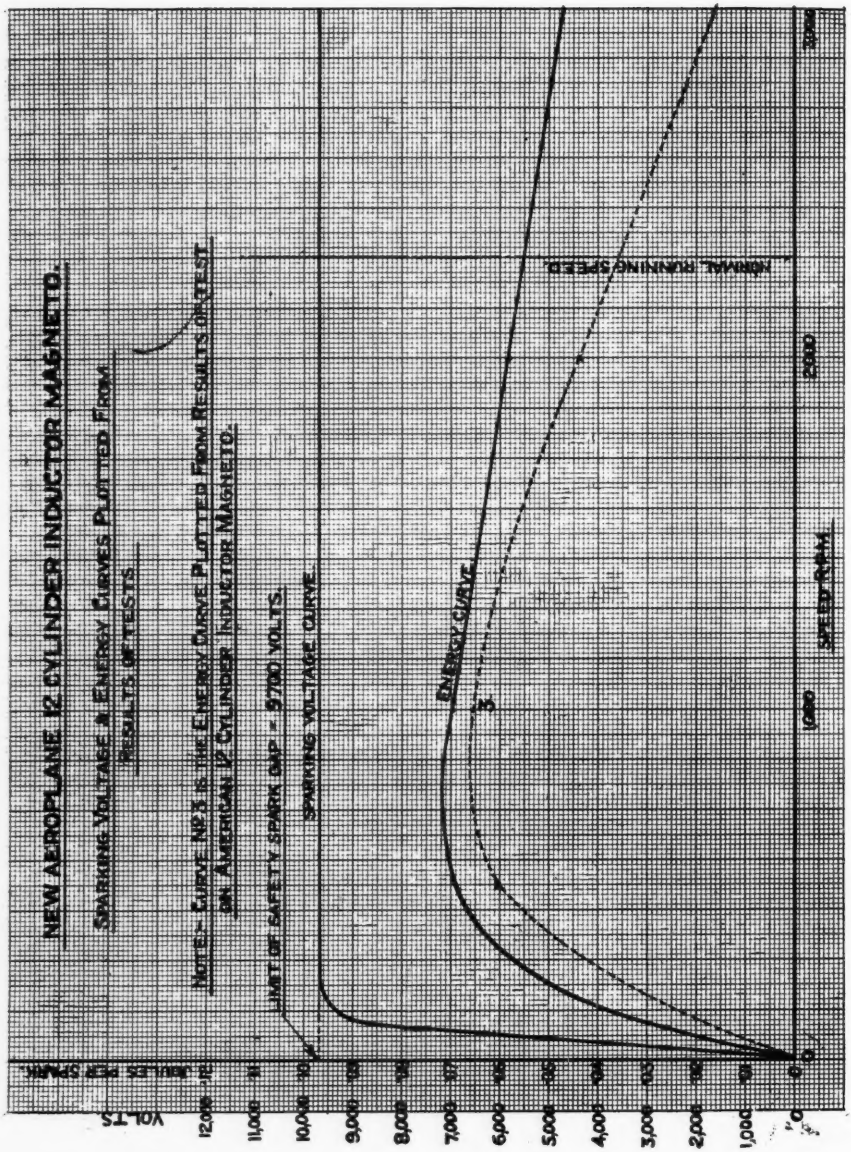

NEW AEROPLANE 12 CYLINDER INDUCTOR MAGNETO.
SPARKING VOLTAGE & ENERGY CURVES PLOTTED FROM RESULTS OF TESTS
NOTE:- CURVE NO 3 IS THE ENERGY CURVE PLOTTED FROM RESULTS OF TEST ON AMERICAN 12 CYLINDER INDUCTOR MAGNETO.
LIMIT OF SAFETY SPARK GAP = 5700 VOLTS.
SPARKING VOLTAGE CURVE.
ENERGY CURVE.
3
NORMAL RUNNING SPEED
VOLTS
SPEED R.P.M.
1000
2000
3000

FIG. 35.

manner any rotating armature type of magneto. When the ratio between the primary and secondary turns is taken into consideration, one is led to the conclusion that at the instant when the high-tension spark is initiated an abnormally high voltage is induced in the secondary, this voltage being greatly in excess of any figure that has been previously assumed.

I have already discussed this particular aspect of the problem at considerable length, and would simply add, therefore, that this result would seem to lend support to the views which I have put forward. Possibly it affords a clue as to the reason why the polar inductor type of magneto appears to possess certain inherent advantages, so far as the igniting power of the spark is concerned, over the rotating armature type as ordinarily constructed.

### (5) Overall Efficiency.

The input, output, and efficiency curves are given in Fig. 37. The input curve has been plotted from the values of the power required to drive the magneto at different speeds as measured by an electrical method, while the output curve is calculated from the spark energy curve given in Fig. 35. The efficiency curve has been calculated in the usual manner, and is substantially in agreement with the curve for the type "A" magneto given in Fig. 31.

## PART 3.

## THE INSTALLATION AND CARE OF A MAGNETO.

### (1) Installation.

Satisfactory operation of a magneto can only result if the alignment between the driving shaft and the magneto shaft be perfect. It is necessary to drive the magneto through some form of flexible coupling, and Fig. 38 shows a coupling that is used very extensively on aeroplane engines. The drive is transmitted through a steel leaf spring "A" having a total cross-section of 11 mm. × 15 mm. Provision is made for obtaining an angular adjustment of the part "B," which grips the leaf spring, with respect to the part "C" secured to the magneto spindle, for the purpose of timing.

This particular design of coupling is not ideal, because whilst being sufficiently flexible in a plane at right angles to the axis of rotation, to take care of rapid and cyclic variations in the driving torque, it does not allow for the alignment of the two shafts being slightly in error. Obviously, any error of this nature will bring into play large stresses which may eventually cause the magneto to function imperfectly. I believe that a large percentage of the troubles that have arisen in practice with aeroplane magnetos have been due to imperfect alignment.

The magneto manufacturer is very careful in machining the base to ensure that the distance between the bottom of the base and the axis of the spindle conforms with extremely fine limits of accuracy. For example, this distance in the type A.8.S. magneto is nominally 50 mm., the limits being plus 0 mm. and minus 0.05 mm., giving a tolerance of 0.05 mm.

I doubt whether engine builders are working to such fine limits as these in milling the platform to which the magneto is secured, and in fact it is a very difficult matter for them to do so. Some improvement is therefore to be looked for in the direction of devising some method of fixing that will simplify the machining problem. The use of a curved base plate on the magneto is a possible solution, because this enables the engine builder to machine the surfaces requiring

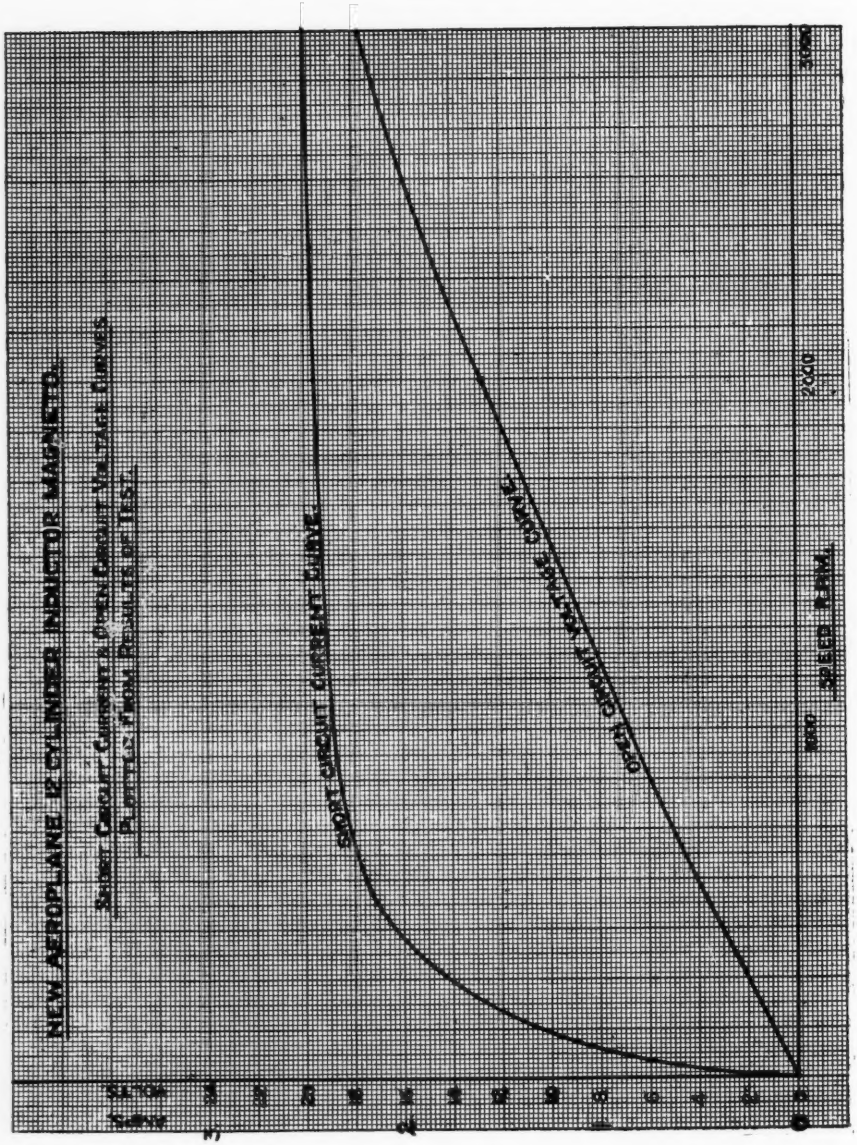

NEW AEROPLANE 12 CYLINDER INDUCTOR MAGNETO
Series Curves Connecting Open Circuit Voltages Curves
& Speed of Armature Results of Test
SPARK GAP IN INCHES
OPEN CIRCUIT VOLTAGE CURVE
SHORT CIRCUIT CURRENT CURVE
SPEED R.P.M.
2000
1000

FIG. 36.

alignment at a single boring operation, instead of having to perform two distinct boring and milling operations.

The curved form of base is used by the S.E.V. Co., of Paris, on one of their rotating armature type six-cylinder magnetos. With this arrangement dowel pins are used in the base for location purposes, the magneto itself being secured by some form of clamping device. This would seem to be an eminently sound arrangement.

Even when fixing screws are used in the base, it is customary to employ a strap which completely embraces the magnets. These straps are usually made of steel, and tests carried out on a type A.8.S. magneto have shown that the shunting of the magnet flux resulting therefrom amounts to about 6%. This means approximately a 12% loss in the energy output of the magneto—an amount not to be overlooked. It is therefore recommended that the straps be made of phosphor bronze, and I believe steps are now being taken to make this change.

**(2) Timing.**

It is, of course, essential that when installing a magneto the adjustment of the coupling should be carefully made to give what is termed the correct "timing." In other words, when the contacts are just on the point of separating —as determined by the indication of a battery and bell connected in parallel with them—the relation of the driving shaft to the magneto shaft must be such that the piston in the cylinder to be fired is some definite distance from the top of its stroke. When the magneto is provided with variable ignition, this adjustment is made with the timing lever fully advanced. Usually, an aeroplane magneto is timed so that each cylinder is fired when the crank shaft is about 30° behind the dead-centre position.

In the type A.8.S. magneto the operation of timing is facilitated by the introduction of locking holes in the distributor brush gear wheel and distributor end plate, so that a pin inserted in the gear wheel can lock the sleeve inductor in the correct position. The pin can only be inserted after the distributor is removed, and likewise the pin must be withdrawn before the distributor can be replaced. This ensures that the magneto is not run with the locking pin inadvertently left in place.

**(3) Setting of Distributor Brush.**

When the contacts are on the point of separating and the high-tension discharge is about to begin, the carbon brush (or metal brush in the type A.8.S. magneto) should overlap the distributor segment to the extent of about two-thirds of its contact surface. In the rotating armature type magneto the desired relation between brush and segment can be easily achieved by altering the meshing of the distributor brush gear wheel with the full-speed wheel on the armature shaft. Generally speaking, the correct meshing is indicated by the magneto manufacturer, who spot-marks two teeth on the brush gear wheel and one tooth on the other wheel. When the armature is in some definite position the latter tooth should mesh between the two former teeth.

In the type A.8.S. magneto the relation of brush to segment can only be altered by displacing the brush holder moulding with respect to the gear wheel to which it is secured. A change in the meshing of the two gear wheels is not permissible.

**(4) Care of Distributor and Brush.**

The distributor and brush are vital parts of any magneto, and in overhauling they should be subjected to very careful treatment. The brush track of the distributor should be kept clean and polished. Any appreciable carbon deposit can be removed by cleaning the track with a cloth soaked in petrol, but if the

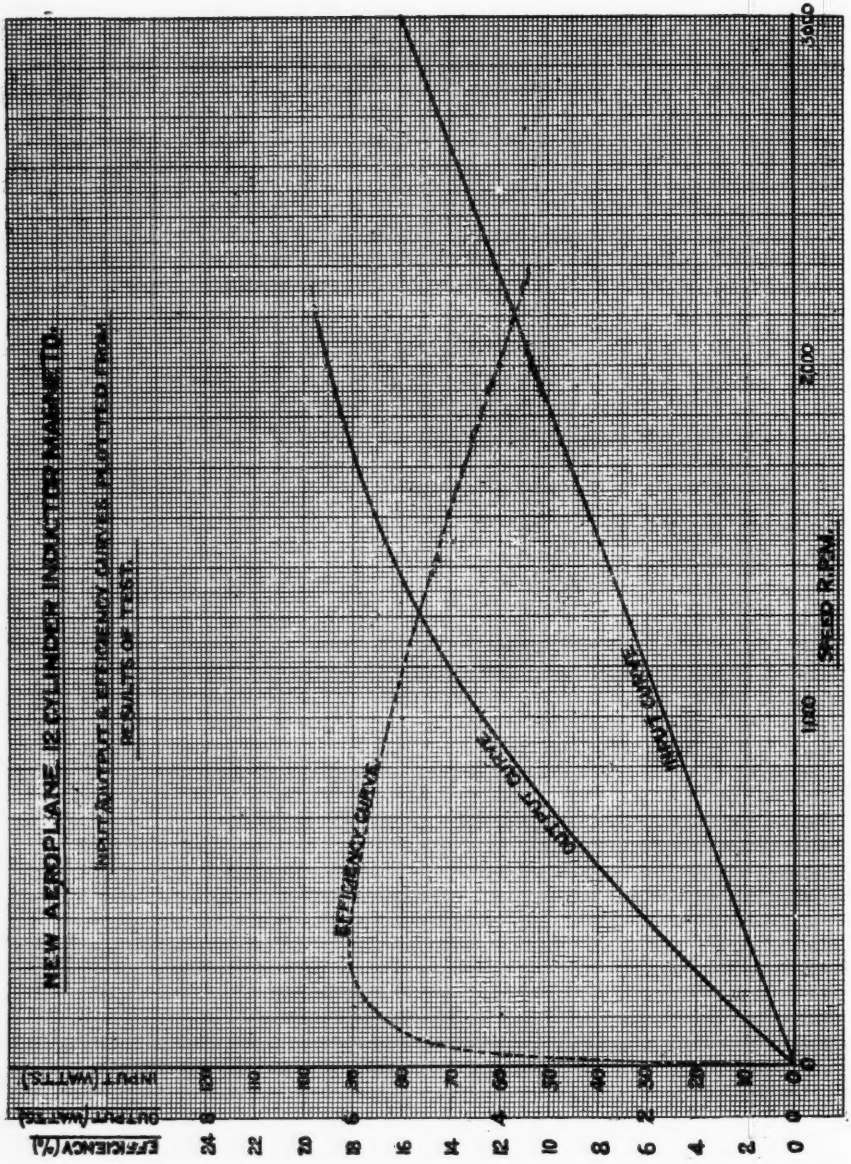
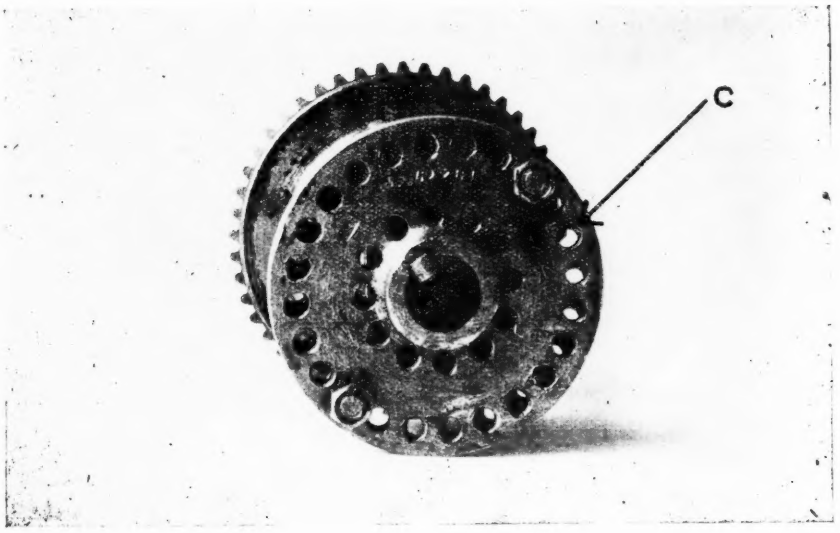

NEW AEROPLANE 12 CYLINDER INDUCTOR MAGNETO.
INPUT, OUTPUT & EFFICIENCY CURVES PLOTTED FROM RESULTS OF TEST.
EFFICIENCY (%)
OUTPUT (WATTS)
INPUT (WATTS)
EFFICIENCY CURVE
OUTPUT CURVE
INPUT CURVE
SPEED R.P.M.
1000
2000
3000

Fig. 37.



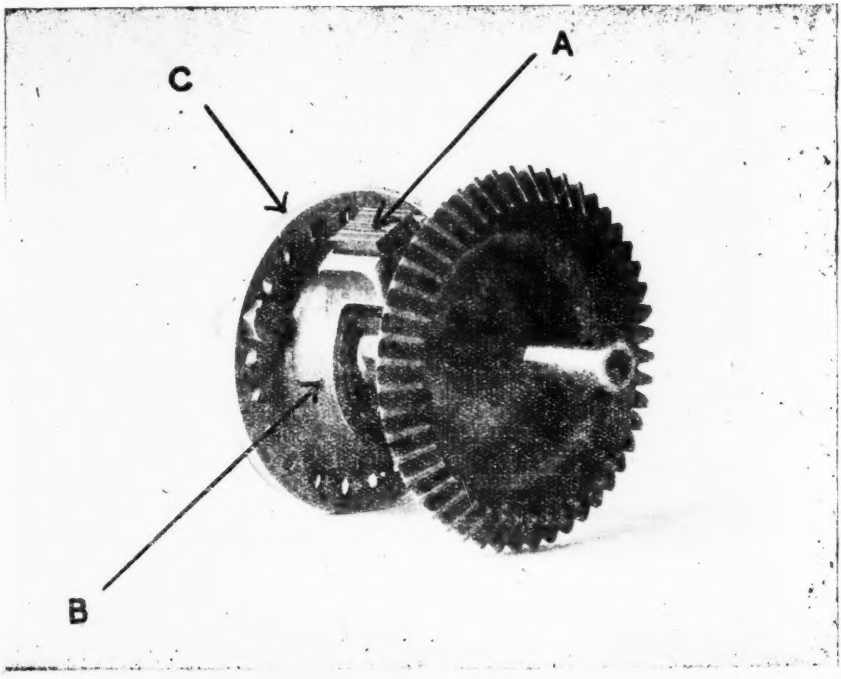




FIG. 38.

segments protrude even by the smallest amount, it is necessary to have the track machined. If the distributor is made of an entirely satisfactory insulating material, trouble of this nature will not be experienced, because there will be no tendency for the insulation to shrink away from the metal during the life of the distributor.

For the best results to be secured, it is necessary to use a very hard grade of carbon brush, which should be properly bedded and polished. The spring tension should not be excessive, and both brush and spring should be perfectly free to move in a radial direction. There should be no signs of binding in the brush holder. I am informed that in certain quarters a carbon brush having a highly-polished contact face is looked upon with suspicion, and in consequence it is the practice to remove the polished surface by the careful application of a file! This is a most pernicious habit, and cannot be condemned too strongly.

Experience has shown that to secure freedom from what is called "tracking" of the distributor it is very necessary to use a carbon brush that will readily take a high polish. The polished skin on the face of the brush is a desirable thing, and, when working in conjunction with a distributor made of suitable material and having a polished brush track, there is no likelihood of "tracking."

**(5) Contact Breaker.**

The satisfactory operation of any magneto is to a very large extent dependent upon the platinum contact breaker points being correctly adjusted. In the type A.8.S. magneto the maximum gap between the contact points, which are made slightly convex, should be .012in. In rotating armature type magnetos the gap usually worked to is .015in., and, generally speaking, a magneto that has to operate at very high speeds—that is, one which has to give a large number of sparks per minute, as in the case of a 12-cylinder magneto—should work with a smaller gap than one that has not to perform such strenuous duties. Great care should be exercised to prevent oil getting on to the contacts, because this will set up sparking, in consequence of the large resistance interposed between the contact faces, and carbonisation of the oil will result, which will greatly interfere with the satisfactory running of the machine. The contacts should therefore be kept perfectly clean, and this can be readily done by using a fine emery cloth. A file should not be employed under any circumstances whatever.

**(6) Lubrication.**

As it is standard practice to use ball bearings in every type of high-tension magneto, very little lubrication is necessary, and special attention should be given to this point by all those who are entrusted with the care of magnetos fitted to aeroplanes or used for other purposes. Over-lubrication is, in fact, more likely to produce injurious results than too little lubrication, because the oil wells are likely to become flooded, and oil will possibly find its way into the distributor and contact breaker housing. Taking the type A.8.S. magneto, for example, six drops of oil in each oil well every 12 hours of running will be quite sufficient.

## THE PROBLEM OF ENGINE STARTING.

The starting characteristics of the petrol motor are inherently bad. With other prime movers, such as the steam engine or turbine, motion results as soon as steam is allowed to enter the machine, whilst the ordinary electric motor can be started by the simple closing of a switch.

There are, fundamentally, only three methods of starting a petrol motor, neither of which is ideal—

(*a*) To rotate the crank shaft slowly by applying a driving torque, until the cylinders are filled with an explosive mixture and the magneto

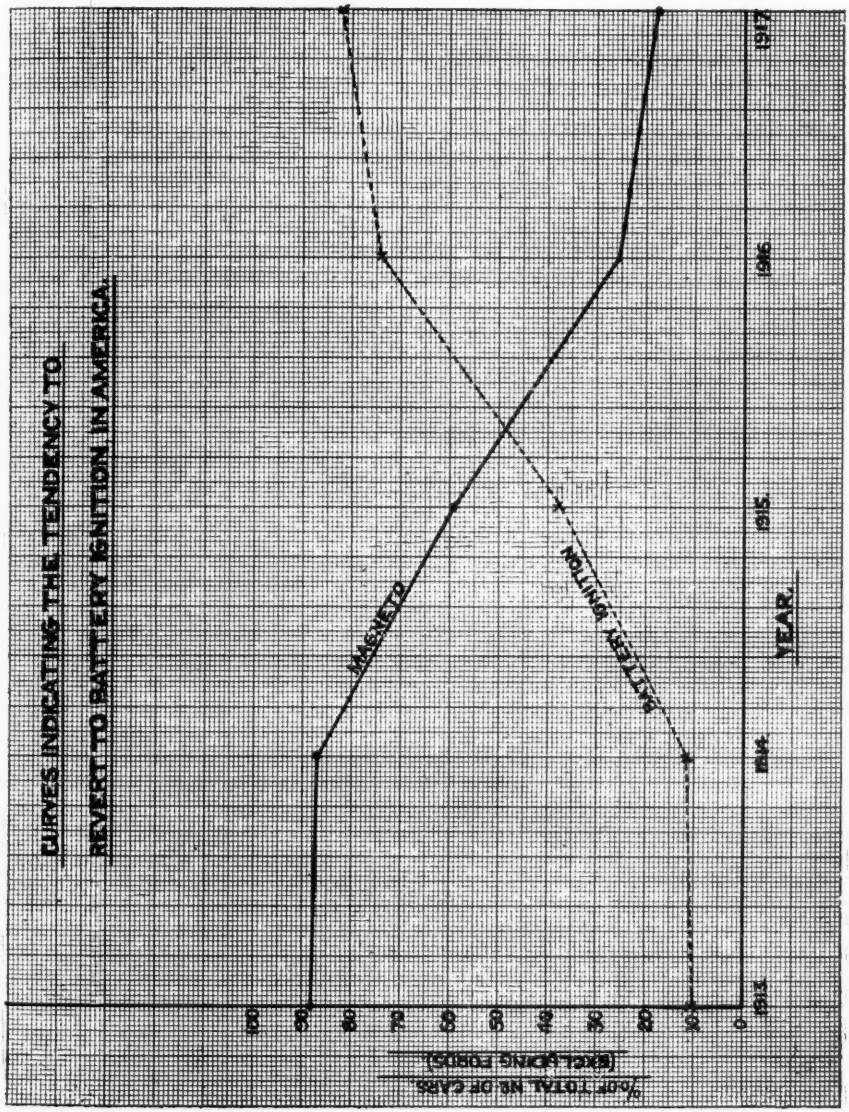

CURVES INDICATING THE TENDENCY TO
REVERT TO BATTERY IGNITION IN AMERICA.
MAGNETO
BATTERY IGNITION
1913
1914
1915
1916
1917
YEAR.
% OF TOTAL NE OF CARS
(EXCEL KING FORDS)
0
10
20
30
40
50
60
70
80
90

FIG. 39.

is running at a sufficiently high speed to ensure satisfactory ignition. The driving torque may be produced either by hand rotation or by driving the crank shaft through gearing with an electric motor fed from a battery.

(*b*) To introduce compressed air into the cylinders so that the necessary driving torque is transmitted to the crank shaft through the medium of the pistons and cylinders. Starting is then effected as under (*a*).

(*c*) To fill one particular cylinder with an explosive mixture by hand rotation, or in some other way, and, whilst the crank shaft is stationary, cause an igniting spark to occur in this cylinder, using for this specific purpose a hand-operated auxiliary magneto co-operating with the main magneto. The main magneto may itself be adapted for producing this spark, while stationary, if suitable auxiliary apparatus be provided.

Method (*a*), using an electric motor, is generally adopted in America at the present time for motor-cars. It is also employed very considerably in this country for the same purpose. The method is far from perfect, chiefly for the reason that the torque required to start the engine is enormous for the size of motor that is generally installed.

The consequence is that at the instant when the switch is closed, the momentary rush of current reaches an extraordinarily high value—about 200 amps. for a 12-volt battery—when considered in relation to the capacity of the battery that has to supply it. Such treatment means that the battery very rapidly wears out and becomes useless.

This method has, I understand, been tried on aeroplanes, but with what success I cannot say. Fundamentally, it does not seem to be a method particularly suited to the requirements of the case, unless by very special attention to the design of the electric motor it is possible to reduce the weight to an extremely small figure.

Method (*b*) has also been tried on aeroplanes, but here again, possibly on account of the increased weight of the compressed-air bottles which have to be carried, the method does not seem to have come into general use.

Method (*c*) appears to have met with the greatest success, and it is the method generally used at the present time on aeroplanes. So that this method can be applied, it is necessary to slightly modify the distributor brush holder, by adding what is termed a "trailing point." This is merely a nickel point which is moulded in the insulator so that the angle between it and the axis of the distributor brush is something of the order of 30°. This point is connected to a brass ring on the front of the brush holder moulding, and an extra terminal on the distributor is connected to an additional carbon brush which is in contact with the ring.

The hand-starting magneto—which is a small rotating armature type machine driven through gearing by hand—is connected to the extra distributor terminal, so that when it is operated the trailing point is maintained at a high potential. This means that when the trailing point comes in close proximity to the trailing edge of the distributor segment current will flow from the point to the segment through the air, and thence to the electrodes of the spark plug in the cylinder that is filled with an explosive mixture.

Owing to the angular displacement between the trailing point and the main brush, the piston in the cylinder under consideration will always be near the dead-centre position when the conditions as previously described obtain. The piston should, therefore, receive a forward impulse when the spark occurs, and this should be followed by other impulses in proper sequence, until a speed is

reached at which the ordinary magneto is capable of performing its proper functions.

The hand-starting magnetos that have been developed in this country differ

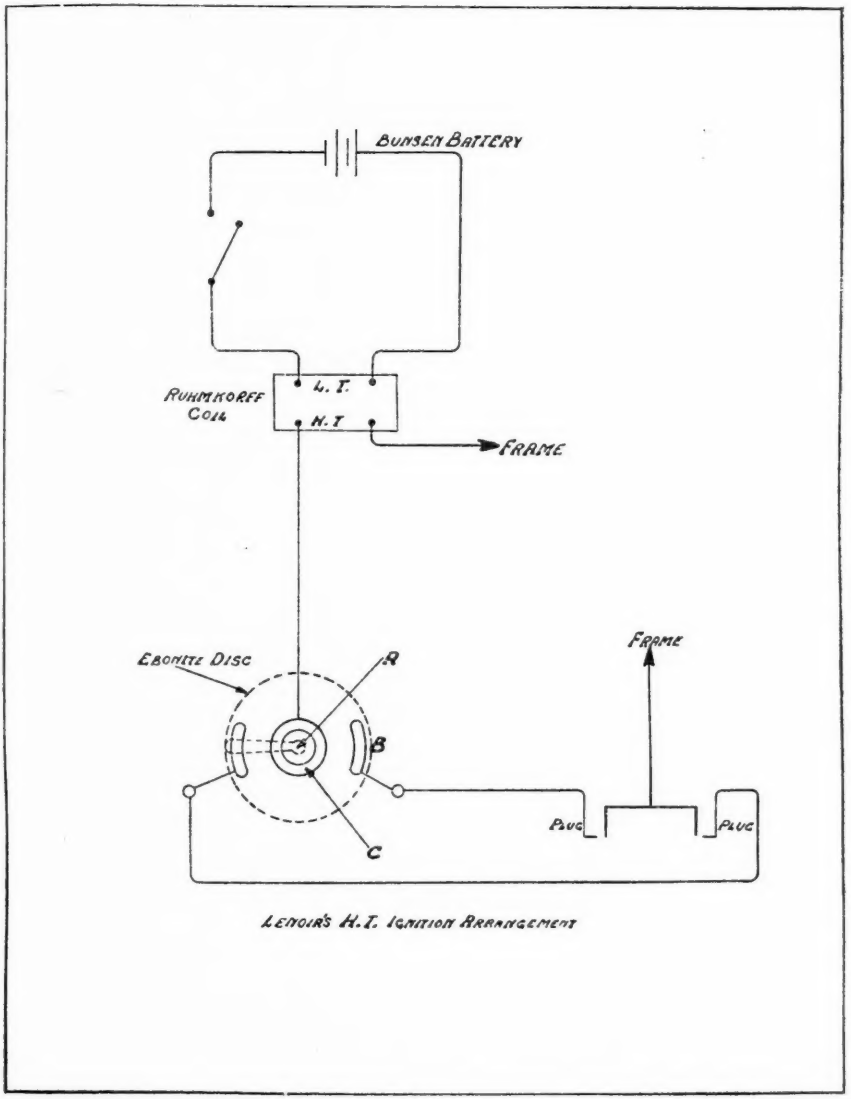
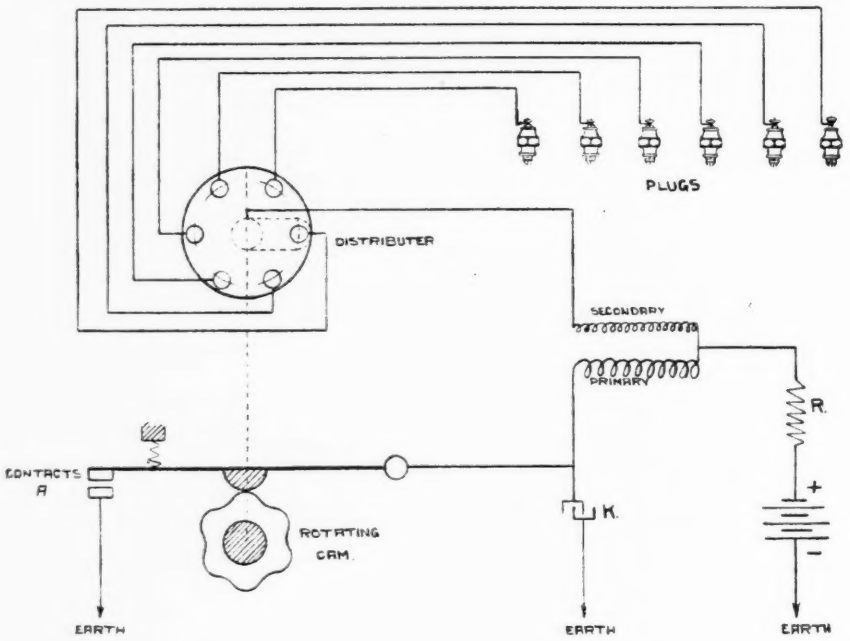



FIG. 40.

from the standard Bosch machine in that no high-tension switch is provided for the purpose of breaking the connections to the main magneto so long as the starting magneto is being operated. It is true that the introduction of such a

device complicates the wiring, because it means that the high-tension lead from the armature of the main magneto has to be brought to a distributor terminal on the starting magneto, and another lead run between some other terminal on this distributor to the rotating brush on the main magneto.

Against all this we have to consider that the use of a high-tension switch absolutely prevents the main magneto coming into action until the starting magneto is stopped. Without the high-tension switch the main magneto will come into action at extremely low speeds, so that to prevent backfires it is necessary to retard the ignition during the starting period. The other scheme




MODERN BATTERY & COIL H.T. SYSTEM
(AS USED IN AMERICA)

FIG. 41.

eliminates this necessity, because with fixed ignition on the main magneto it should be possible to take the engine up to a sufficiently high speed with the hand starter to ensure that when the main magneto comes into operation the timing of the spark will not be too far advanced.

It is perhaps not out of place to refer to the method of starting adopted on the 200 h.p. Maybach engine fitted to German airships brought down in this country, as described in " The Autocar " for December 16th, 1916. This type of engine has six cylinders, two-point ignition being provided. Two Bosch Z.H.6 magnetos—a new type of rotating armature magneto evidently developed since the outbreak of war—one of which is fitted with a trailing point, are provided.

The hand starter is coupled up to the magneto with trailing point in the ordinary way.

A special method is adopted for filling the cylinder with an explosive mixture. By means of a lever and cam shaft all the valve tappets, inlet and exhaust, are lifted off their cams. At the same time the lever actuates other levers which close the exhaust passage to the silencer, by partially rotating the water-cooled barrel valve in the exhaust manifold. The exhaust ports and the outlet in the exhaust manifold are thus put in communication with a secondary outlet pipe that leads to a large suction hand pump, by means of which gas is sucked into the cylinders from the two carburetters through all the inlet valves. When this has been done the lever arm previously referred to is returned to its normal position. This operation opens the exhaust outlet to the silencer in the normal running position and closes the pipe leading to the suction pump. The hand starter is finally turned and the engine begins to rotate.

It is interesting to note that the angle between the trailing point and the centre of the distributor brush on the Bosch Z.H.6 magneto is 42°. On the type A.8.S. magneto fitted with trailing point this angle is, as already stated, 30°.

## SPECIAL PROBLEMS IN CONNECTION WITH THE DESIGN OF AEROPLANE MAGNETOS.

### (1) Reliability.

Reliability is a desirable feature of every piece of apparatus, no matter for what purpose it be used. In the case of an aeroplane magneto, this is a consideration of primary importance, because failure of the magneto may mean sudden destruction of the machine and pilot.

Fundamentally, a reliable piece of apparatus will result if :—

(1) The design is reliable.
(2) The materials are correctly chosen.
(3) The workmanship is perfectly sound.

These three points can be checked by *testing* the apparatus most carefully with the object of bringing to light any incipient defect.

We therefore see very clearly that an aeroplane type of magneto should be most carefully and rigorously tested before it leaves the manufacturer. During this test the strain put upon the machine, both mechanical and electrical, should be greatly in excess of the maximum strain likely to obtain under abnormal working conditions. If the machine passes this test, both the manufacturer and user will know that the factor of safety is sufficient to ensure that no breakdown will occur during the subsequent life of the machine.

These fundamental considerations were carefully borne in mind when arranging the series of tests to be imposed upon the type "A" magneto. Furthermore, an elaborate system is in force for testing samples from each batch of material as received from outside suppliers, so as to ensure that the rigid specifications to which materials are ordered are faithfully adhered to.

In the case of the armature, which forms such a vital part of the whole construction — being virtually the "heart" of the magneto — extraordinary precautions are taken. First, by carefully inspecting all the insulating material and each reel of enamelled wire, and secondly, by subjecting the finished armature to a prolonged electrical test with the secondary discharging at 9,500 volts between electrodes. In addition to this, the armature is inspected several times at various stages of the winding and finishing process.

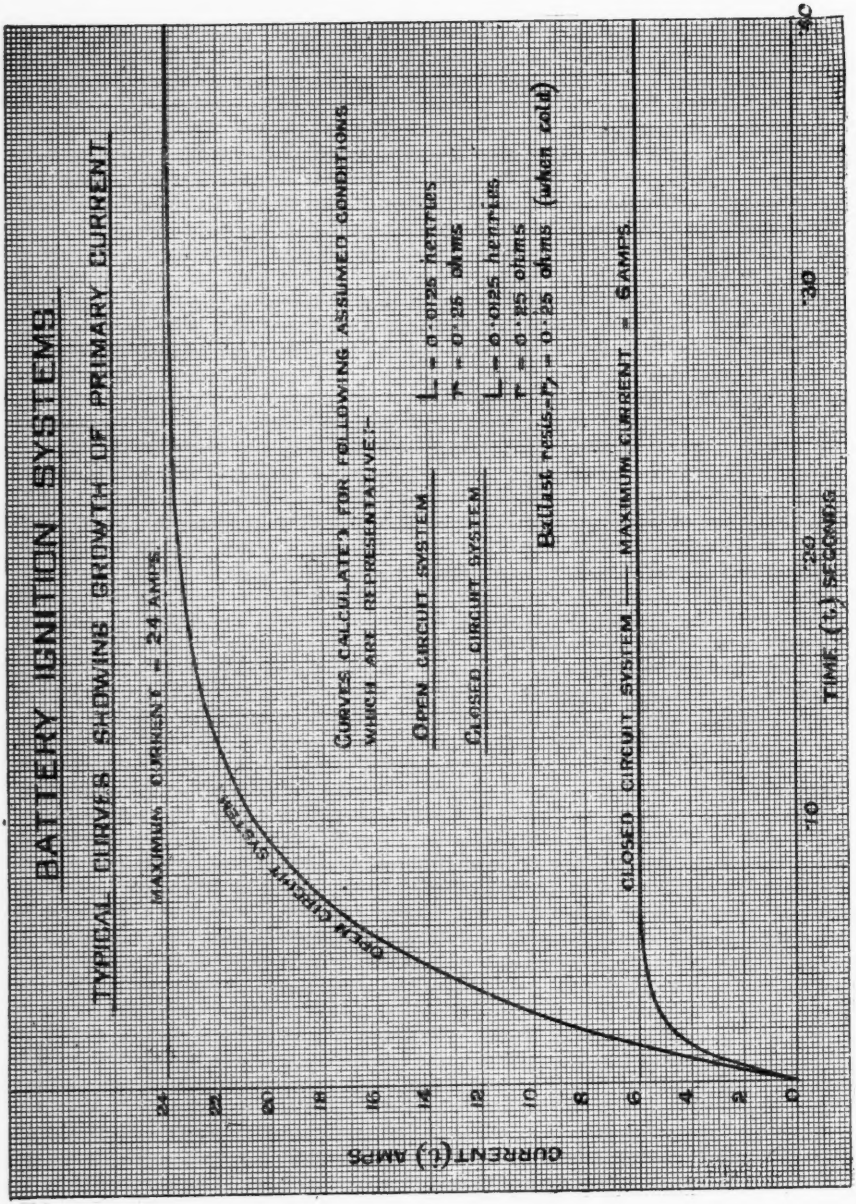

BATTERY IGNITION SYSTEMS
TYPICAL CURVES SHOWING GROWTH OF PRIMARY CURRENT
MAXIMUM CURRENT = 24 AMPS
OPEN CIRCUIT SYSTEM
CURVES CALCULATED FOR FOLLOWING ASSUMED CONDITIONS WHICH ARE REPRESENTATIVE:-
OPEN CIRCUIT SYSTEM
L = 0·0125 henries
r = 0·25 ohms
CLOSED CIRCUIT SYSTEM
L = 0·0125 henries
r = 0·25 ohms
Ballast resis. r' = 0·25 ohms (when cold)
CLOSED CIRCUIT SYSTEM —— MAXIMUM CURRENT = 6 AMPS.
CURRENT (i) AMPS
TIME (t) SECONDS
0
2
4
6
8
10
12
14
16
18
20
22
24
10
20
30

FIG. 42.

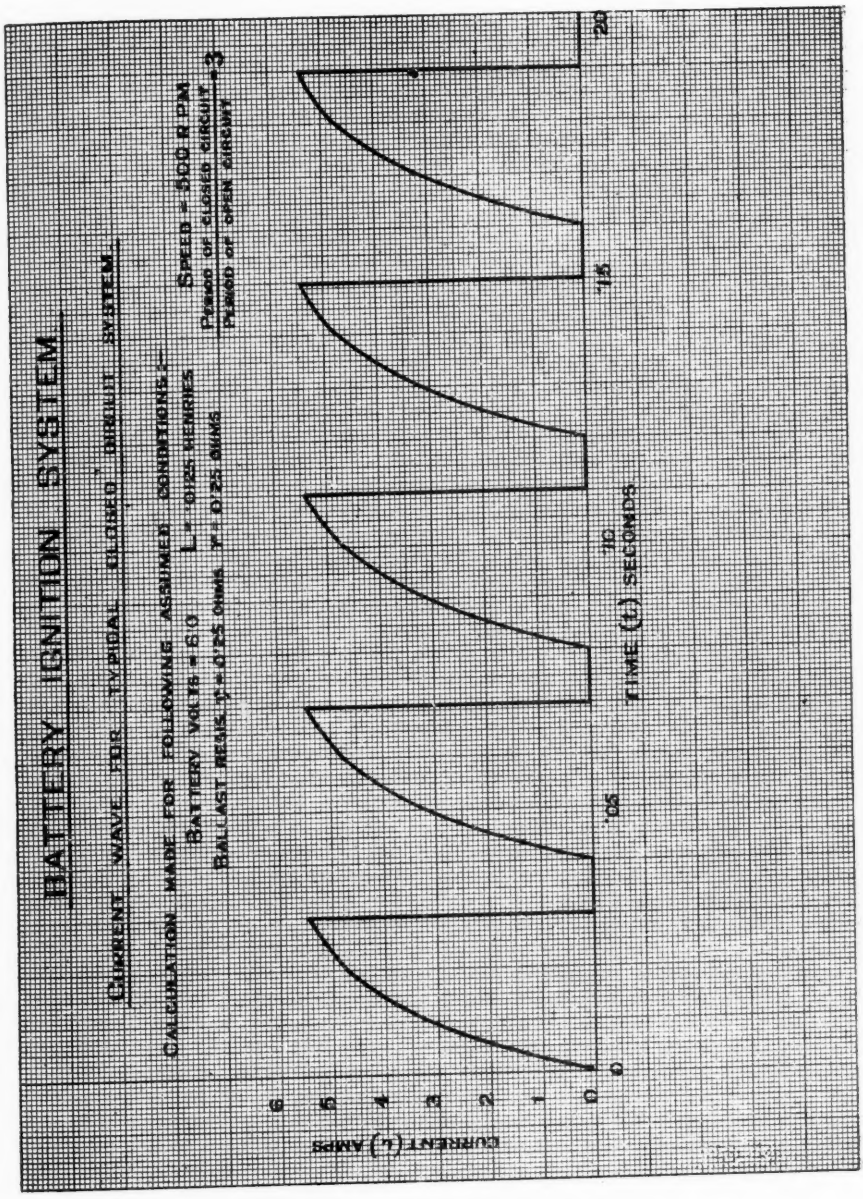

BATTERY IGNITION SYSTEM.
CURRENT WAVE FOR TYPICAL CLOSED CIRCUIT SYSTEM.
CALCULATION MADE FOR FOLLOWING ASSUMED CONDITIONS:—
SPEED = 500 R.P.M.
BATTERY VOLTS = 6.0 L = 0.25 HENRIES
Period of closed circuit = .3
BALLAST RESIS. r = 0.25 OHMS R = 0.25 OHMS
Period of open circuit = .3
CURRENT (i) AMPS
0
1
2
3
4
5
6
0
.05
.10
.15
.20
TIME (t) SECONDS

FIG. 43.

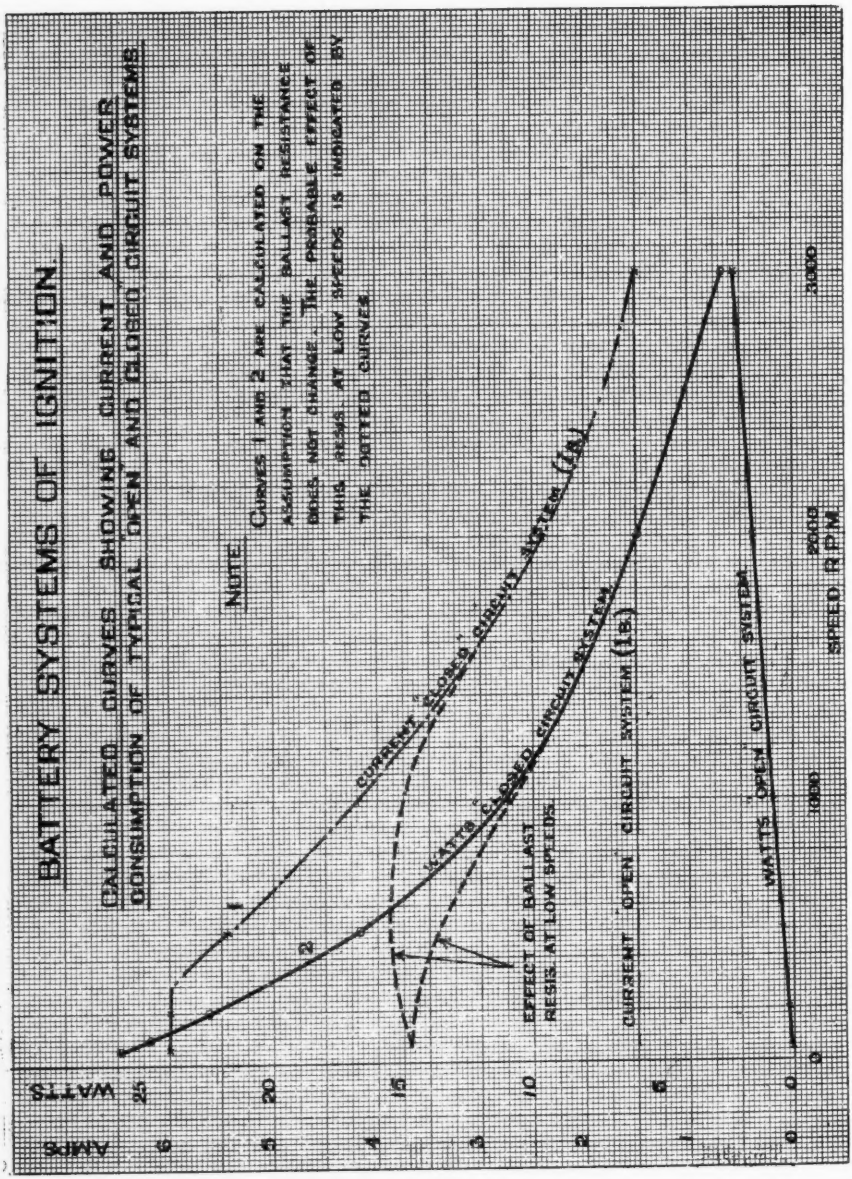

BATTERY SYSTEMS OF IGNITION
CALCULATED CURVES SHOWING CURRENT AND POWER CONSUMPTION OF TYPICAL 'OPEN' AND CLOSED' CIRCUIT SYSTEMS
NOTE
CURVES 1 AND 2 ARE CALCULATED ON THE ASSUMPTION THAT THE BALLAST RESISTANCE DOES NOT CHANGE. THE PROBABLE EFFECT OF THIS RISE AT LOW SPEEDS IS INDICATED BY THE DOTTED CURVES
1
2
CURRENT 'CLOSED' CIRCUIT SYSTEM (1A)
WATTS 'CLOSED' CIRCUIT SYSTEM
EFFECT OF BALLAST RISE AT LOW SPEEDS
CURRENT 'OPEN' CIRCUIT SYSTEM (1B)
WATTS 'OPEN' CIRCUIT SYSTEM
AMPS
WATTS
SPEED R.P.M.
1000
2000
3000

Fig. 44.

Finally, every magneto is subjected to running tests as follows :—

(1) Run for 20 hours at 2,500 r.p.m. at air temperature, with each distributor terminal connected to a spark gap adjusted to discharge at 7,500 volts.

(2) Completely dismantled and various metal parts cleaned in petrol. Magneto then reassembled and adjustments made where necessary.

(3) After re-magnetising, magneto run for four hours at 2,500 r.p.m., with spark gaps as under (1), in a hot box with air at 100° F.

(4) At the end of this final run the magneto is tested at low speeds to ensure that the sparking characteristics are satisfactory.

(5) Before shipment, the magneto is inspected in every detail, all adjustments carefully checked, and screws tightened and locked where necessary.

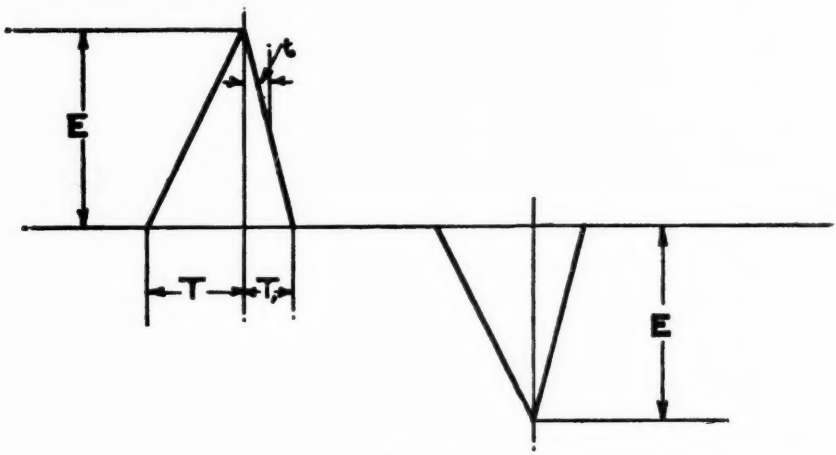



FIG. 45.

**(2) Reduction of Weight.**

It is obviously desirable to keep the total weight of an aeroplane type magneto as low as possible. To do this, it is first necessary to develop a design that requires a minimum volume of material, and secondly, to use, if possible, aluminium castings. Special steels should also be used wherever possible, so that the required strength can be achieved with a minimum weight of material.

Die castings made of an aluminium-copper alloy containing no zinc are to be preferred to sand castings, because the general appearance of the magneto is enhanced thereby, and the amount of machining that has to be performed on the parts is very considerably reduced. Furthermore, the metal is much tougher, and the threads in tapped holes are not so likely to become stripped when the screws are securely tightened.

At the present moment the few manufacturers in the country capable of producing satisfactory die castings for this class of work are so inundated with urgent Government orders that it is doubtful whether they are in a position to meet the whole of the requirements of the magneto industry. I refer to the matter, however, in the hope that the many firms now producing satisfactory aluminium sand castings, but not die castings, may have their interest stimulated in this direction.

Most excellent die castings are at the present time used by all the leading magneto manufacturers in France, judging by the French magnetos that have come into my hands, and it would appear that the aluminium die-casting industry in France is in a very healthy state of development.

It is evident, therefore, that great possibilities are presented to English manufacturers, because for small repetition work every designer would prefer to specify accurately-finished die castings, rather than sand castings, if there were reasonable hopes of obtaining adequate and continuous supplies in this country.

**(3) Elimination of Fire Risk.**

This is of vital importance, because any sparking within the magneto may, under operating conditions, be the means of initiating a fire by igniting a mixture of air and petrol in the surrounding medium. I am at liberty to refer to certain tests* that were carried out by the N.P.L. in the early days of the war to determine the extent of this danger. A number of different types of pre-war Bosch magnetos were taken, including the types Z.U.4, Z.F.4, D.U.6, and D.A.L., and run in a chamber that could be filled with an explosive mixture of air and petrol. Generally speaking, it was found that explosions were likely to occur as a result of :—

(1) A succession of sparks at the safety gap.
(2) Sparks between distributor segments or other high-tension parts, and the frame of the magneto.
(3) Sparks at the contact-breaker points.

Trouble is most likely to arise from cause (1), although this was eliminated by enclosing the safety spark gap in a fine mesh gauze casing on the principle of the Davy safety lamp, so as to prevent any explosion in the immediate vicinity of the safety gap electrodes being propagated in the surrounding medium.

Trouble due to causes (2) and (3) can be eliminated by careful attention to the design of the magneto to prevent any wasteful sparking of this nature. In the type " A " magneto this is done, whilst the safety spark gap is contained in a separate gas-tight chamber fixed underneath the distributor and provided with a ventilation hole covered with a piece of very fine mesh gauze. It is necessary to ventilate the chamber so that the products of ionisation have free access to the surrounding air. The gauze opening ensures that any explosion occurring inside the chamber as a result of a discharge at the safety gap is not communicated to the outside medium.

As a " spark-gap " type of distributor is used, it is necessary to provide some form of ventilation so that the products of ionisation can escape. Here again the ventilation hole in the front of the distributor is covered with a piece of fine mesh gauze, and the whole combination is thus perfectly safe. I would add that very severe tests carried out on this particular machine, by introducing an explosive mixture of coal gas and air into the contact-breaker housing, safety spark-gap chamber, and distributor, have quite confirmed this view that the risk of fire under operating conditions has been entirely eliminated.

In the 12-cylinder polar inductor magneto previously described, the safety spark gap rotates with the distributor brush. This is, in my opinion, a distinct improvement upon the stationary form of spark gap generally adopted, because the products of ionisation are churned up and quickly expelled through the ventilation window provided in the front of the distributor. This window is covered with gauze, as in the type " A " machine, which makes the magneto fireproof.

* See Report and Memoranda No. 186 of Advisory Committee for Aeronautics.

**(4) The Problem of Variable Timing.**

Opinions appear to differ as to whether it is absolutely essential that an aeroplane magneto should be provided with means for adjusting the timing. Several thousand type "A" eight-cylinder aeroplane magnetos with fixed ignition have been supplied, so presumably on certain types of engines variable ignition is not required.

When starting a petrol engine it is, generally speaking, necessary to retard the ignition so as to prevent the explosive wave in the cylinder being sufficiently propagated before the piston reaches the top of its stroke to produce a backfire.

In the case of an aeroplane engine, the necessity for making such an adjustment should not arise if a hand-starting magneto provided with H.T. switch be used. Reference is made to this point under "The Problem of Engine Starting." In view of the fact that the hand starters now being used in this country are not fitted with this special cut-out switch, it seems necessary that some provision should be made either inside the magneto or in the outside drive for an adjustment of this nature. The method of obtaining adjustment of the timing, adopted by the Bosch Company in their pre-war rotating armature type magnetos, was to make provision for rocking the contact-breaker cam housing through a definite angle ranging between 16° and 35°. A suitable lever, securely attached to the cam housing, was provided to enable any adjustment of this nature to be easily made. Obviously, any angular displacement of this housing will cause the spark to occur either earlier or later in each cylinder, depending upon whether the lever receives a backward or forward movement.

The primary current curves shown in Figs. 23 and 24, which are typical of what occurs in other types of magnetos, clearly prove that at low speeds—when the necessity for retarding the ignition is greatest—any retarding of the spark will greatly weaken its intensity. At high speeds, on the other hand, the reverse is true, and the spark becomes stronger as the timing lever is retarded.

This inherent defect of a H.T. magneto, as ordinarily constructed, can be overcome by providing special sliding poles, which are mechanically connected to the timing lever, so that their position is changed at each adjustment of the timing. A design of this kind is used on one of the Bosch Co.'s latest magnetos, developed since the outbreak of war, and it ensures that when the primary circuit is broken, the armature always bears a definite relation to the pole pieces, regardless of the position of the timing lever. In consequence, the current $I_B$ for any given speed will remain constant, and the intensity of the spark will be the same for all adjustments of the timing.

Having overcome one inherent difficulty, the other one associated with the distributor still remains. If we assume that when the timing lever is fully advanced the contact-breaker points close just before the carbon brush leaves the distributor segment—and at very high speeds the H.T. spark will persist until the contacts close—it is clear that with the lever fully retarded the carbon brush will be some distance off the segment when the spark ends. In other words, sparking between carbon and segment will result, which must tend to burn the brush track.

This second inherent difficulty can only be overcome by designing the contact-breaker cams to give a very short period of open circuit ($\theta_1$), but from other considerations it is undesirable to make $\theta_1$ too small. The difficulties likely to arise in this connection are more pronounced on a 12-cylinder magneto, where the problem of distribution is very complex.

Taking everything into consideration, therefore, I firmly hold the view that in the case of an aeroplane magneto adjustment of the timing should be made outside the magneto and incorporated in the drive by the engine designer. This enables the magneto designer to work out his design on the basis of fixed

ignition, and he is thus enabled to simplify the mechanical construction of the machine and arrange matters so that the magneto will give the very best electrical performance over a wide range of speed.

The problem of devising some mechanical method of adjusting the timing that can be incorporated in the magneto drive is not difficult of solution. For example, some form of sleeve working on a coarse thread and capable of movement in an axial direction can be provided for this purpose. On these lines it is possible to develop a scheme whereby both magnetos (in cases where two-point ignition is adopted) can be adjusted in phase by the movement of a single-lever arm. It is also possible to provide for adjustment through a worm spindle operated by a flexible drive terminating in a hand wheel placed in close proximity to the pilot. This latter method would allow of a remarkably fine adjustment being obtained.

It is hoped that a solution of this problem will be found by working along these lines, and that eventually the magneto designer need not concern himself with the problem of providing as an integral part of the magneto, means for timing adjustment.

## THE PROBLEM OF SECURING A TRULY BRITISH MAGNETO.

Now that a British magneto industry has been established on a sound foundation, it is vitally important that when the war is over this industry should be supplied with the necessary materials used in the construction of high-tension magnetos from within the British Empire. Only in that manner can we secure a truly British-made magneto. Viewed from the national standpoint only, such a course is absolutely essential, because otherwise, in the event of war, this industry, upon which so many other war industries are dependent, will be handicapped for lack of one or more essential materials.

So that this problem can be better appreciated by those in authority, I itemise below the various materials now being used in the construction of high-tension magnetos, classifying, where possible, under the component parts of the machine, in the construction of which they are used.

**(1) Armature.**

(*a*) Enamelled high conductivity copper wire.
(*b*) Varnished paper, silk, and cambric.
(*c*) Silk tubes of very fine bore.
(*d*) Best Egyptian cotton tape.
(*e*) Insulating baking varnish.
(*f*) High resistance sheet-iron giving maximum permeability.

**(2) Condenser.**

(*a*) Best ruby mica.
(*b*) Tin foil, preferably having a matt surface on both sides.

**(3) Contact Breaker.**

(*a*) Best grey fibre.
(*b*) Platinum-iridium contacts.
(*c*) Very high-grade steel strip springs.

**(4) Magnets.**

**(5) Moulded Insulating Material comparable with Stabalite.**

**(6) Ebonite Rod and Sheet.**

**(7) Carbon Brushes.**

**(8) Aluminium-Copper Die Castings.**

**(9) Brass Stampings and Die Castings.**

**(10) Special Steels.**

(*a*) Accurately finished stampings.
(*b*) Rough forgings.
(*c*) Bar material.

**(11) Ball Bearings.**

Practically all the essential materials itemised above are produced in this country at the present time, but I would call your special attention to the following materials with a view to stimulating the interest of English manufacturers in regard to this important question, so as to ensure that there will be no doubt whatever about the magneto industry being able to obtain adequate supplies in this country after the war is over.

**Varnished Paper, Silk, and Cambric.**

These insulating materials are essential to the manufacture of a satisfactory magneto armature, and although they can be purchased in England at the present moment, I believe I am right in saying that the greater portion of this material is imported from America. I am also informed that the varnished silk in particular is of somewhat peculiar origin. The very fine mesh silk itself is woven in Japan, being sent from there to America, where it is treated with a special high-grade insulating varnish. The finished article is then sent to England, where it is distributed by agents to the various magneto manufacturers. There does not seem to be any reason why English silk manufacturers should not supply silk of the necessary degree of fineness, and if this is done I understand that there is no difficulty in subjecting the silk to a varnish treatment that will produce a finished article comparable with what is now being imported from America.

There is one other point in connection with this matter worthy of note. The insulating varnish generally used for this purpose has a linseed oil base, and the varnish manufacturer is at the moment depending on supplies obtained from foreign countries. As is well known, linseed oil is produced by crushing the seeds of the flax, and it is evident, therefore, that adequate supplies could be obtained within the boundaries of the British Empire.

**Moulded Insulating Material comparable with Stabalite.**

At the present moment there are several manufacturers in England who are making moulded insulating material similar to the German stabalite, and although very good results have been achieved, it does not appear that these manufacturers are yet in a position to meet all the demands of the magneto industry for moulded distributors and other parts, such as slip rings and brush holders. Furthermore, an improvement in the quality of the material now being produced is to be looked for, although I would add that the recent progress made in certain directions has been most gratifying.

There seems absolutely no reason to suppose that the English manufacturers will not eventually be in a position to adequately meet all the demands for this class of moulded material, but co-operation between them and the magneto manufacturers is necessary, so that the former may be fully alive to the importance of details, and to ensure that immediate action is taken to overcome

any objectionable characteristic of the material, which may only make itself evident under operating conditions on a magneto.

**Carbon Brushes.**

I believe it is a fact that the most satisfactory distributor carbon brushes are at the moment being manufactured in France, although one English manufacturer in particular has certainly made very commendable progress during the past 12 months in the direction of producing a distributor brush comparable with the French brush in question. It is hoped, therefore, that the English manufacturers of carbon brushes will in the very near future be able to meet all the demands made upon them by English magneto manufacturers. This is another case where hearty co-operation between the brush manufacturer on the one hand and the magneto manufacturer on the other hand is necessary if the best results are to be achieved.

**Die Castings.**

(*a*) *Aluminium.*

For magneto work, die castings made from an alloy of aluminium and copper, containing no zinc whatever, are eminently desirable.

Satisfactory castings of this composition are at the present time being produced in this country, although unfortunately the amount of war work to be done in other directions does not allow of the few manufacturers concerned meeting all the demands of the magneto manufacturers. It is hoped, however, that the manufacturers of die castings will give special attention to this question immediately, so as to ensure that after the war is over the demand for die castings that will be made upon them by English magneto manufacturers will be fully met.

(*b*) *Brass.*

The present position as regards the manufacture of brass die castings is even less satisfactory than that outlined above. I would, therefore, point out that the magneto industry would receive considerable assistance if it were possible to purchase brass die castings in large quantities, and it is hoped that the question will receive the serious attention of the manufacturers concerned in the immediate future.

**Conclusion.**

In conclusion, I should like to tender my thanks to my assistants, Mr. H. Warren and Mr. F. G. Coombes, for the very considerable help they have rendered in connection with the experimental work referred to in the text and the preparation of curves. Also, I am indebted to the firms of Thomson & Bennett and the M.L. Magneto Syndicate for the information included in this paper concerning certain of their aeroplane magnetos. Finally, I have to thank the B.T.H. Company (with whom I am connected), who have very generously allowed me to make such free use of certain information gained in their Experimental Laboratory, and which forms the basis of most of what I have written on this subject.

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## APPENDIX I.

BY A. P. YOUNG, A.M.I.E.E., A.I.A.E.

## SOME NOTES CONCERNING BATTERY IGNITION SYSTEMS.

### (1) THE USE OF THE SYSTEM IN AMERICA.

During recent years the battery system of ignition has become extremely popular in America, and at the present time it is even being used more extensively for automobile work than the high tension magneto. I have therefore felt that it would not be out of place to include, in the form of an appendix to my paper which primarily deals with H.T. Magnetos, a detailed analysis of the working of the battery system. In Fig. 39 I give two curves which show what the American practice has been since 1913. The percentages which form the basis of these curves relate to all types of American car, excluding the Ford, and they have been compiled by the " Horseless Age " of America.

| | Percentage of cars fitted with :— | |
|---|---|---|
| Year. | Magneto. | Battery ignition. |
| 1913 ... | 88.1 ... | 10.7 |
| 1914 ... | 87.3 ... | 11.9 |
| 1915 ... | 59.5 ... | 37.9 |
| 1916 ... | 25.4 ... | 74.4 |
| 1917 ... | 17.77 ... | 82.23 |

The figures given for 1917 are taken from an article in the " Horseless Age " for September 1st, 1916, and were deduced from information available at that time. It is possible, therefore, that the actual percentages may be slightly different to the figures quoted. In any case one must conclude that the battery system of ignition is still favourably looked upon in the States, and there has been during the past three years a most marked tendency in the direction of this system of ignition displacing the high tension magneto. On the other hand, it must be carefully borne in mind that so far American users have only had about two years' extended experience with the battery system of ignition.

### (2) MAGNETO *versus* BATTERY SYSTEM.

This very vexed question around which a heated controversy has been raging both here and in America for some time past, will undoubtedly solve itself in the course of time, and if during the next few years the magneto curve in Fig. 39 reaches a minimum and takes an upward turn it will clearly show that the battery system has been fairly put upon its trial and found wanting.

I do not imagine for a moment that the battery system will ever die out entirely in America, because owing to the vogue of electric lighting and starting in that country there are many good arguments which can be put forward for a thoroughly reliable system of this kind. At the same time it must be fully realised that the battery system of ignition has been used on American cars to such a large extent in recent years simply because there was not available in that country a high tension magneto comparable with the Bosch magneto, manufactured in Stuttgart, Germany, which, as you all know, was generally used on practically every English car prior to the outbreak of war. No doubt the American high tension magnetos now being manufactured are a great improvement on those produced a year or so ago, and this fact in itself will have an enormous influence upon the future possibilities of a battery system of ignition in that country.

My own view is that even in this country after the war we are likely to see some form of battery ignition system at least tried—if not standardised—for certain English cars, and this possibility should be faced by all manufacturers of

high tension magnetos. Having said this, however, I feel convinced that the extent to which a battery system of ignition is adopted by English motor car manufacturers after the war, will depend greatly upon the efforts that are put forth by the English magneto manufacturers in the direction of producing a very cheap, and at the same time a very reliable, high tension magneto. If they succeed in doing this, then I do not think that the battery system of ignition will come into extensive use, because, on the basis of the two ignition outfits being sold at the same price, the magneto is undoubtedly the superior article. It is self-contained and a thoroughly reliable unit, whereas no matter how the battery system is constructed this must essentially be dependent for its efficiency upon a separate unit, namely, the battery, which unfortunately is the poorest mechanical and electrical component part of the modern motor car.

The battery system of ignition has been suggested for use on aeroplanes. I consider that the use of a battery system of ignition for this particular purpose is a retrograde move, because in aeroplane construction reliability—even regardless of cost—should be the first and primary consideration. From the reliability standpoint the battery system is inherently weaker than the high tension magneto, for in the one case, as stated above, the ignition system derives its power from a separate unit, namely, the battery, which is very liable to get out of order, whilst in the other case the magnets, which are thoroughly reliable, both mechanically and magnetically, are the source of energy.

Bearing these facts in mind, therefore, I do not think that the use of a battery system of ignition for aeroplane work should be seriously considered so long as the English magneto manufacturers are in a position to produce thoroughly reliable high tension magnetos, as they have been doing since the outbreak of war.

## (3) AN ANALYSIS OF THE WORKING OF THE BATTERY SYSTEM OF IGNITION.

### (*a*) Historical.

The modern battery system of ignition is a direct descendant of the first high tension system of electrical ignition which was devised and constructed by Lenoir about 1860. The arrangement of Lenoir's system is shown in Fig. 40.

A bunsen battery supplied current to an ordinary Ruhmkorff induction coil, and the secondary winding of this was connected to plugs through a high tension distributor. The latter consisted of a rotating insulated arm (*a*), which swept over a brass insulated ring (*C*) and two brass insulated segments (*b*). The rotating arm (*a*) did not make metallic contact with either the ring or segments, and the current in flowing to the plugs had to jump the small intervening air space between the under side of the arm (*a*) and the ring, on the one hand, and between the arm and the segment on the other.

It is interesting to note that the modern battery system is constructed on much the same lines, except that the trembler make and break on the induction coil has been replaced by a positive make and break mechanically operated. In the early days of motoring in this country the battery coil system was generally used, the current to a trembler coil being controlled by a rotating contact disc. Most of the troubles experienced in those early days appear to have been due to the trembler coil, and in the very latest systems as used in America this device is dispensed with and a positive make and break, as used in the magneto, is employed instead.

### (*b*) The Modern Battery System.

Fig. 41 represents diagrammatically the arrangement generally employed at the present day. A description of this diagram will suffice to explain the main

features of all the battery systems now being used in America, because these systems differ only in their constructional details and generally in respect of the design of the distributor and contact breaker.

The induction coil does not call for special comment. It is constructed on the lines of the well known device designed and perfected by the French Mechanician Ruhmkorff sixty years ago, except that the integral contact breaker mechanism is dispensed with. The primary consists of a few turns of thick wire, while the secondary is wound with a large number of turns of very fine wire, the ratio of turns being something of the order of 100. Both windings are threaded on to an iron core, the primary being nearest the iron and the secondary on the outside of the primary. A condenser "K" and ballast resistance "R" are fitted to the coil, and the whole forms one complete unit.

The ballast resistance is sometimes dispensed with, although in the "closed circuit" system it is necessary to limit the current through the primary at low speeds. Iron wire is most frequently used for this unit, because it increases its resistance rapidly with increase in temperature. Should the current tend to rise, the increased heating in the resistance unit will result in a rapid rise in resistance, thus automatically preventing any appreciable change in the current.

The primary winding, it will be noted, is connected in series with the resistance "R" and the contacts "A," direct across the battery, while the secondary winding is coupled up to the rotating arm of the distributor in the usual manner. In fact, if the battery resistance "R" be deleted so that the primary is short circuited on itself when the contacts "A" close, the connections correspond exactly to those of the H.T. Magneto. The only vital difference between the working of the two systems is that whereas in the magneto the current in the primary is "induced" by the rotation of the armature in a magnetic field, in the other system current is supplied by the battery at a practically constant voltage.

Usually the contact breaker and distributor form one complete unit, the contact breaker cam and distributor arm being fitted to the same spindle. It is necessary with some systems to provide for automatic "advance" of the spark with increase in speed, and this result is achieved by driving the contact breaker cam shaft through a spring controlled centrifugal governor, so that it receives a forward displacement (with respect to the driving shaft) which increases with the speed.

The contacts "A," which in many cases are made of tungsten, are first closed for a certain period—during which time the current in the primary grows in value—and then suddenly opened. At that instant the H.T. spark occurs as a result of the enormous voltage induced in the secondary by the collapsing of the magnetic field created by the primary current. The intensity of the H.T. spark is dependent—other things being equal—on the value of the primary current that is broken when the contacts "A" separate. Let us therefore investigate this point a little more closely.

### (c) **Theoretical Considerations.**

When the contacts close a steady and constant voltage "E" is applied to an inductive circuit which comprises the primary winding in series with a certain resistance, and the current grows from zero in conformity with the well known exponential law:—

$$i = \frac{E}{R}\left\{1 - \epsilon^{\frac{-Rt}{L}}\right\} \quad . \quad . \quad . \quad . \quad (1)$$

Where

$i$ = Instantaneous current—Amps.
$R = r + r_1$ = Total resistance of primary circuit—Ohms.

$L$ = Self-induction of primary—Henries.
$E$ = Battery voltage.
$t$ = Time in seconds, measured from the instant at which the circuit is closed.
$r$ = Resistance of primary—Ohms.
$r_1$ = Ballast resistance—Ohms.

During the short interval of time that the primary current is growing, a magnetic field is being created by the current and it is by virtue of this magnetic field that energy is stored in the winding. At the instant of "break" the magnetic field suddenly collapses and induces in the secondary winding an enormous voltage which is sufficient to produce a spark at the plug. The action is analogous to what takes place in a H.T. Magneto, although the characteristics of a coil can be much more easily investigated because we are dealing with a steady voltage the whole time, whereas in a magneto the voltage is alternating and dependent on the speed of rotation of the armature.

Looking at the matter in another way, one may say that at the instant of "break" the energy stored magnetically in the primary at a low potential is transferred instantly to the secondary at a very high potential, where it manifests itself as a spark at the plug points. Of course, a certain amount of energy is lost during the process, but probably something like 70% of the energy stored in the primary is available for ignition.

In considering the characteristics of different ignition coils, therefore, we are (as with the H.T. Magneto) chiefly concerned with the primary self-induction "L" and the value of the current at "break" ($I_B$). The latter can be readily calculated for known conditions, from equation (1), and I can best explain the working of this formula by applying it to a concrete example.

Assume that

$L = 0.0125$ henries.
$R = 0.25$ ohms.
$E = 6$ volts.

The growth of the current in the primary circuit under these conditions is shown by the larger exponential curve in Fig. 42, calculated from equation (1). The current ($i$), theoretically, never attains the steady value ($I$) as calculated from Ohm's Law, which in this case is 24 amps, as the current curve is asymptotic with respect to the chain-dotted horizontal line corresponding to the steady current. At the end of 0.3 seconds, however, it can be assumed that the steady condition has been reached, because at that instant the current curve is, for all practical purposes, merged with its asymptote.

If we consider a time interval of only 0.0033 secs. measured from the instant when the circuit is closed, we see that the current has reached a value of 1.5 amps. In other words, the current has reached 6.2% of its final steady value in less than one hundredth of the time taken for that steady condition to be attained. The rate at which the current grows is very rapid at the instant of closing the circuit, but the rate of growth diminishes as time goes on. Obviously, the whole shape of the curve can be altered by changing ($L$) and ($R$); that is, by altering the size of wire and the number of turns on the primary; or by inserting a ballast resistance in series with the winding. In this connection it should be remembered that for a given design of coil:—

$$L = K \times S_P^2$$
$$r = K_1 \times S_P^2$$

Where

$K$ and $K_1$ = Constants.
$S_P$ = Primary turns.

Let us suppose that a ballast resistance of 0.75 ohms is connected in series with the coil just considered, bringing the total resistance of the primary circuit up to 1 ohm. The maximum current ($I$) is now reduced to 6 amps. The smaller

curve in Fig. 42 represents the growth of the current under these new conditions. Considering either of these curves, it will be found that the ratio $L/R$ (usually called the " time-constant " ($K$) of the circuit) is the time taken for the current to reach 63.2% of its final and steady value ($I$).

### (*d*) The Vital Importance of the Contact Breaker.

The form of curve to be aimed at is settled by the characteristics of the contact breaker mechanism that is used in series with the primary winding. Broadly speaking, these mechanisms can be classified under two distinct groups :—

(1) The " closed circuit " contact breaker.
(2) The " open circuit " contact breaker.

The " closed circuit " contact breaker corresponds in principle to the mechanism adopted on many high tension magnetos, in that the circuit is closed for the greater portion of the time interval between successive sparks. At the instant of firing the circuit is rapidly broken and then immediately made again. The type of breaker mechanism used in the Delco and Connecticut systems belongs to this class. In such cases it is not essential that the current curve be abnormally steep at the beginning, and the lower curve in Fig. 42 would be suitable. In fact, the values of ($L$) and ($R$) chosen as a basis for this curve are quite representative of the " closed circuit " battery system used in America at the present day, so that the current values given by the curve are fairly accurate for such a system.

Considering the case of a 6-cylinder engine running at 2,000 r.p.m., 100 breaks per second would be required; that is, the interval between successive breaks would be 0.01 seconds. Assuming that the ratio :—

$$\frac{\text{Time circuit is closed}}{\text{Time circuit is open}} = 3.0$$

then the primary is closed for 0.0075 seconds at this speed, and open for 0.0025 seconds. This means that at the instant of " break " the primary current has reached the value of 2.4 amps. At lower speeds, the time during which the primary is closed would be correspondingly greater. Thus, at 1,000 r.p.m. the primary would be closed for 0.015 secs., and at " break " the current would be 4.2 amps. At very low speeds, say below 200 r.p.m., the maximum current ($I$) would be broken in every case.

With the " open circuit " type of contact breaker mechanism as used in the Attwater-Kent system,* the primary circuit is open for practically the whole time, the contact breaker being of the quick-acting type designed to rapidly make and break the circuit, just before firing. It is claimed that in the Attwater-Kent system the primary circuit is closed for only 0.0033 seconds. This time interval is, furthermore, independent of the speed.

It is obvious that when the " open circuit " type of contact breaker is used, the current curve must be very steep at the beginning, so that the current in the primary can reach an appreciable value before the circuit is again broken. The large curve in Fig. 42 is suitable for these conditions, and it should be noted that if the primary be closed for 0.0033 seconds, the current will have reached 1.5 amps. This is the current ($I_B$) that would be broken. Here again, the assumed conditions are quite representative of the " open circuit " system as used in America.

* The Attwater-Kent Co. have now changed over to a " closed circuit " type of contact breaker, although they have supplied enormous quantities of battery ignition sets fitted with their well-known type of " open circuit " contact breaker.

**(e) The Electrical Characteristics of the System.**

(1) *Energy Consumption.*

I have already explained that during the period when the contacts are closed the primary current rapidly increases from zero value in accordance with a definite law. After a certain time interval, the current is suddenly broken, and this usually occurs before the final and steady value (as calculated from Ohm's Law) is reached. To determine the energy consumed by the primary in a given time, therefore, we must measure the area of a series of current curves which take the general form shown in Fig. 43, and multiply this result by the battery voltage.

Let us assume that:—

Time during which contacts are closed $= T_1$
Engine speed ... ... ... ... $= n$ r.p.m.
Number of cylinders ... ... ... $= a$
Number of breaks per sec. ... ... $= \frac{na}{120} = b$

and adopting the notation previously used, we get:—

Each current wave shown in Fig. 42 conforms with the law of growth:—

$$i = I\left(1 - \epsilon^{\frac{-Rt}{L}}\right)$$

$$\therefore \text{ Area of each current wave} = \int_0^{T_1} i.dt$$

$$\text{Area of each current wave} = I\int_0^{T_1}\left(1 - \epsilon^{\frac{-Rt}{L}}\right).dt$$

Calling this area $X$ we have:—

$$X = I\int_0^{T_1} dt - I\int_0^{T_1} \epsilon^{\frac{-Rt}{L}}\, dt$$

$$X = I.T_1 + I.\frac{L}{R}\left(\epsilon^{\frac{-RT_1}{L}} - 1\right)$$

$$X = I\left\{T_1 + \frac{L}{R}\left(\epsilon^{\frac{-RT_1}{L}} - 1\right)\right\}$$

The energy consumed by the primary in one second is given by:—

$$\text{Energy consumed} = E_G = X \times b \times E \text{ Joules.}$$

This must also equal the rate of consumption, or the power ($W$) expressed in watts. Therefore:—

$$\text{Power} = W = XbE \text{ watts,}$$

and as a final result:—

$$W = IE\left[\frac{na\left\{T_1 + K\left(\epsilon^{\frac{-T_1}{K}} - 1\right)\right\}}{120}\right] \quad . \quad . \quad . \quad (2)$$

The expression in brackets is a factor which is dependent on the type of contact breaker used, and the speed $n$. Calling this factor $\eta$ we get :—

$$\text{Watts consumed by coil} = W = I.E \times \eta.$$

In other words, the drain on the battery, expressed in watts, is under operating conditions always less than the power ($EI$) that would be taken by the coil if it were connected permanently to the battery, by an amount depending on the factor $\eta$.

This question is of vital importance, and I can best illustrate the value of equation (2) by using this formula to determine the power, for different speeds, in the case of :—

(*a*) A "closed circuit" system, assuming representative figures as already given.

(*b*) An "open circuit" system, assuming, as under (*a*), the representative figures to which I have already referred.

## CASE 1. CLOSED CIRCUIT SYSTEM.

$L = .0125$ henries.
$R = 1$ ohm.
$E = 6$ volts. $I = 6$ amps. $EI = 36$ watts.

$$\text{Ratio} \frac{\text{Time primary is closed}}{\text{Time primary is open}} = 3.0 = k$$

$a = 6.$
$K = .0125.$

$$T_1 = \frac{.75 \times 120}{na} = \frac{15}{n} \text{ secs.}$$

Calculating $\eta$ for different speeds, and tabulating, we get :—

| Speed r.p.m. | $T_1$ secs. | $I_B$ amps. | $\eta$. | Watts taken by coil. |
|---|---|---|---|---|
| 50 | 0.3 | 6 | .72 | 25.9 |
| 100 | 0.15 | 6 | .688 | 24.7 |
| 200 | 0.075 | 6 | .625 | 22.5 |
| 500 | 0.03 | 5.45 | .465 | 16.7 |
| 1000 | 0.015 | 4.2 | .307 | 11.04 |
| 2000 | 0.0075 | 2.4 | .175 | 6.3 |
| 3000 | 0.00375 | 1.5 | .075 | 2.7 |

## CASE 2. OPEN CIRCUIT SYSTEM.

$L = 0.0125$ henries.
$R = 0.25$ ohms.
$E = 6$ volts. $I = 24$ amps. $EI = 144$ watts.
$T_1 = .0033$ seconds for all speeds.
$K = 0.05.$

Calculating $\eta$ for different speeds as before, and tabulating, we get :—

| Speed r.p.m. | $T_1$ secs. | $I_B$ amps. | $\eta$. | Watts taken by coil. |
|---|---|---|---|---|
| 50 | .0033 | 1.5 | .00026 | 0.0375 |
| 100 | ,, | ,, | .00052 | 0.075 |
| 200 | ,, | ,, | .00104 | 0.15 |
| 500 | ,, | ,, | .0026 | 0.375 |
| 1000 | ,, | ,, | .0052 | 0.75 |
| 2000 | ,, | ,, | .0104 | 1.5 |
| 3000 | ,, | ,, | .0156 | 2.25 |

The figures given above, which have been computed from hypothetical cases, to bring out the distinctive difference between the two systems, are plotted in Fig. 44.

Curves showing the watts consumed, indicate clearly that:—

(*a*) The power consumption with an "open circuit" system is extremely small as compared with a "closed circuit" system.

(*b*) Whereas the power increases with the speed in the first case, the reverse is true in the second case.

It should be pointed out that the curve for a "closed circuit" system is based on the assumption that the ballast resistance $r_1$ has not changed its value. Actually, at very low speeds the excessive heating will be sufficient to practically double the value of this resistance so that the power consumption is automatically cut down. The dotted curve indicates approximately the relationship between power and speed, under normal operating conditions, and although the power decreases slightly with the speed, the effect of the ballast resistance is to straighten out the curve very considerably.

When considering the overall efficiency of a battery system it must be remembered that the energy consumed by the coil is derived from a battery which in turn stores up a certain amount of energy supplied by a dynamo driven by the petrol motor. That is, two units are introduced between the engine and the coil in each of which a considerable loss of energy occurs, and it is safe to assume that if the power taken by the coil under given conditions is "*W*," then the power that has to be supplied by the engine corresponding to this condition will be something of the order of "2*W*."

For example, in the case of the "closed circuit" system already considered, the watts taken by the coil at 1,000 r.p.m. are approximately 11. This means that at this particular speed the watts taken from the engine would be roughly 22. Added to this we have the relatively large amount of power required to drive the gearing and distributor brush and cam spindle in the actual igniter, which increases with the speed. When it is remembered that a 4-cylinder magneto of good design takes something of the order of 50 watts to drive it at maximum speed, and that the energy output in such a case is very much greater than that furnished by any battery system, it will be appreciated that from this efficiency standpoint—taking the efficiency to be the ratio $\frac{\text{Energy given out by sparks}}{\text{Energy consumed by ignition apparatus}}$—the magneto is superior to any form of battery system.

(2) *Secondary Voltage and Spark Energy.*

The induced secondary voltage at "break" is primarily dependent on the current broken ($I_B$) and the self-induction of the primary winding. For a given coil, and assuming a sparkless "break," the secondary voltage will increase almost in strict proportion to $I_B$. It is likely that in most systems the design of the coil is such that the sparking voltage compares favourably with that produced by a H.T. Magneto of good design.

When we come to consider the energy dissipated by the spark, however, comparison with a magneto will show that the latter is by far superior. Particularly is this true of the "open circuit" system, which of necessity delivers a very feeble spark at the plug. I have already commented on the fact that with this system the drain on the battery is very small. The physical significance of this is that the primary circuit, during each closure, stores up such a small amount of energy that the secondary spark cannot possibly be intense. Taking the hypothetical case just considered we get the following figures:—

$I_B = 1.5$ amps.
$L = 0.0125$ henries.

Now the maximum energy available (assuming no losses) is given by $\frac{1}{2}LI_B^2$ joules. This works out at .0141 joules.

The energy delivered in the H.T. spark is considerably less than this figure, say not more than 0.010 joules. A well designed magneto gives a spark energy at high speeds of the order of 0.05 to 0.10 joules, and whilst with the "open circuit" system the spark energy remains constant over the whole range of speed, a magneto gives a spark that becomes more and more energetic and flamy as the speed increases—at least up to a fairly high speed beyond which the spark intensity is not of such vital importance.

This is an inherent property of the magneto not possessed by either battery system, and one that enables the magneto to give more satisfactory results under working conditions. Experience has shown that a spark which increases its intensity (and flame) with the speed, is to be desired, because it eliminates the necessity of advancing the spark, by automatically speeding up the propagation of the explosive wave.

In the "closed circuit" system, a more energetic spark can be obtained, particularly at low speeds, but the intensity gradually falls off with increase in speed, which is just the opposite to what is required for the very best results.

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## APPENDIX II.

## FORMULA FOR COMPUTING THE PRIMARY CURRENT IN A MAGNETO AT "BREAK."

In dealing with the type A8S magneto, I make use of a graphical method, that I devised some two years ago, of determining from the open circuit primary voltage wave, the primary current curve, and thus the value of the primary current ($I_B$) at break. I have since had an opportunity of going more closely into this matter, and by assuming a definite triangular form for the voltage wave, which can be done without introducing any considerable errors, I have developed mathematically a definite formula for the current $I_B$.

Fig. 48 shows the form of the assumed triangular alternating voltage wave, and in the majority of cases this assumption can be made. I have actually applied this method to the type A8 magneto already considered, and the values of the current $I_B$ calculated for different speeds given in the table which follows the derivation of the formula, check up very closely with the values obtained by the graphical method previously employed. This same method of analysis could be applied to any form of magneto, providing that sufficient data is available to enable one to define the various constants involved.

### DERIVATION OF FORMULA.

**NOTATION.** (See Fig. 45.)

$E$ = Maximum voltage.
$T$ = Time for voltage to grow from zero to the maximum value. Secs.
$T_1$ = Time for voltage to die away from the maximum value to zero. Secs.

$$K = \text{Constant} = \frac{E}{T}$$

$$K_1 = \text{Constant} = \frac{E}{T_1}$$

$R$ = Resistance of primary. Ohms.
$L$ = Self induction of primary. Henries.
$i$ = Instantaneous value of primary current. Amps.
$e$ = Instantaneous value of primary voltage.
$t$ = Time interval in seconds corresponding to current ($i$) and voltage ($e$).

## (1) FIRST PERIOD $O$ TO $T$ SECONDS.

Fundamentally

$$e = Ri + L \cdot \frac{di}{dt} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

During this first period, $e$ increases in direct proportion to $t$. Therefore

$$e = K.t$$

Substituting in equation (1) we get:—

$$Kt = Ri + L \cdot \frac{di}{dt}$$

Or

$$\frac{di}{dt} + \frac{R}{L} \cdot i = \frac{K}{L} \cdot t$$

This is one of the simplest forms of differential equation, and the solution can be at once written down as follows:—

$$i = \epsilon^{\frac{-R}{L}\int dt} \left\{ \int \epsilon^{\frac{R}{L} \cdot dt} \frac{K}{L} \cdot t.dt + K_2 \right\}$$

Or

$$i = \epsilon^{\frac{-Rt}{L}} \left\{ \frac{K}{L} \int \epsilon^{\frac{Rt}{L}} \cdot t.dt + K_2 \right\}$$

Now,

$$\int \epsilon^{\frac{Rt}{L}} \cdot t.dt = \frac{L}{R^2} \cdot \epsilon^{\frac{Rt}{L}} (Rt - L)$$

And substituting, we get

$$i = \epsilon^{\frac{-Rt}{L}} \left\{ \frac{K}{R^2} \cdot \epsilon^{\frac{Rt}{L}} (Rt - L) + K_2 \right\}$$

When $t = o$, $i = o$, and by substitution in the above equation we can determine the unknown constant $K_2$. We get:—

$$K_2 = \frac{KL}{R^2}$$

Finally,

$$i = \frac{K}{R^2} \left\{ L (\epsilon^{\frac{-Rt}{L}} - 1) + RT \right\} \quad . \quad . \quad . \quad . \quad (2)$$

This equation enables the current ($i$) to be calculated for any conditions, during the period when the voltage is increasing to its maximum; that is, approxi-

mately up to the point when the rotating member reaches the "dead centre" position. The primary current is always broken *after* that critical position has been reached, and as the above formula does not apply to this second period, we must pursue our investigation further by considering what happens when the voltage begins to fall, using equation (2) as a starting point for this final investigation.

## (2) SECOND PERIOD, $T$ TO $T_1$ SECONDS.

We can write:—

$$e = E - K_1 t$$

Where

$t$ = Time interval in seconds measured from the instant when the voltage $e$ is a maximum and equal to $E$.

Fundamentally we get:—

$$(E - K_1 t) = Ri + L \cdot \frac{di}{dt} \quad . \quad . \quad . \quad . \quad (3)$$

or

$$\frac{di}{dt} + \frac{R}{L} \cdot i = \frac{E - K_1 t}{L}$$

The solution of this differential equation takes the form:—

$$i = \epsilon^{\frac{-R}{L}\int dt} \left\{ \int \epsilon^{\frac{R}{L}\cdot\int dt} \frac{E - K_1 t}{L} \cdot dt + K_3 \right\}$$

or

$$i = \epsilon^{\frac{-Rt}{L}} \left\{ \frac{E}{L} \int \epsilon^{\frac{Rt}{L}} \cdot dt - \frac{K_1}{L} \int \epsilon^{\frac{Rt}{L}} \cdot t.dt. + K_3 \right\}$$

Now

$$\int \epsilon^{\frac{Rt}{L}} \cdot t.dt = \frac{L}{R^2} \epsilon^{\frac{Rt}{L}} (Rt - L)$$

and by substitution we get:—

$$i = \epsilon^{\frac{-Rt}{L}} \left\{ \frac{E}{R} \cdot \epsilon^{\frac{Rt}{L}} - \frac{K_1}{R^2} \epsilon^{\frac{Rt}{L}} (Rt - L) + K_3 \right\}$$

or

$$i = \frac{1}{R} \left( E + \frac{K_1 L}{R} - K_1 t \right) + K_3 \epsilon^{\frac{-Rt}{L}} \quad . \quad . \quad . \quad (4)$$

To determine the value of the constant $K_3$, we must assume that $t = 0$ and substitute in equation (4) the value of the current ($i$) corresponding. Obviously, this current can be deduced from equation 2 (for the first period) by substituting $T$ for $t$.

Therefore, when $t = 0$,

$$i = \frac{K}{R^2} \left\{ L \left( \epsilon^{\frac{-RT}{L}} - 1 \right) + RT \right\}$$

Substituting these values for $t$ and $i$ in equation (4) we get:—

$$K_3 = \frac{KT}{R} - \frac{KL}{R^2} - \frac{K_1 L}{R^2} - \frac{E}{R} + \frac{KL}{R^2} \cdot \epsilon^{\frac{-RT}{L}}$$

Finally, substituting this value of $K_3$ in equation (4) gives us the following result:—

$$i = \frac{1}{R^2}\left[ ER - K_1 Rt + K_1 L + L.\epsilon^{\frac{-Rt}{L}} \left\{ K.\epsilon^{\frac{-RT}{L}} - (K_1 + K) \right\} \right] \qquad (5)$$

This equation will give the current at any time $t$, and provided the time interval (call it $T_2$) corresponding to the instant of "break" is known, the current at "break" ($I_B$) can be easily calculated. We get as a final result:—

$$I_B = \frac{1}{R^2}\left[ ER - K_1 RT_2 + K_1 L + L.\epsilon^{\frac{-RT_2}{L}} \left\{ K.\epsilon^{\frac{-RT}{L}} - (K_1 + K) \right\} \right] \qquad (6)$$

It is now an easy matter to determine the value $t$ (call it $T_3$) corresponding to which $i$ attains its maximum value. For this condition

$$\frac{di}{dt} = 0$$

Differentiating equation (5) and equating to zero, we get the following formula for $T_3$:—

$$T_3 = \frac{L}{R}\left[ \text{Log}_\epsilon \left\{ \frac{K}{K_1}\left( 1 - \epsilon^{\frac{-Rt}{L}} \right) + 1 \right\} \right] \quad . \quad . \quad . \qquad (7)$$

## APPLICATION OF METHOD TO B.T.H. TYPE A8S MAGNETO PREVIOUSLY CONSIDERED.

Constants are:—

$$t = \frac{5}{n} \qquad R = 0.55 \text{ ohms.} \quad L = 0.012 \text{ henries.}$$

$$T_1 = \frac{2.5}{n} \qquad n = \text{speed r.p.m.}$$

$$T_2 = \frac{0.75}{n} \qquad K = 0.004\, n^2 \times \text{correcting factor.}$$

$E = .0185\, n \times$ correcting factor. $\quad K_1 = 0.008\, n^2 \times$ correcting factor.

Correcting factor:—

| | | |
|---|---|---|
| 100 | r.p.m. | = 1.025 |
| 500 | ,, | = 1.02 |
| 1000 | ,, | = 1.00 |
| 2000 | ,, | = 0.90 |

The following values have been calculated for different speeds:—

| Speed r.p.m. | | $I_B$ Calculated from equation (6). | | $I_B$ By graphical method previously used. |
|---|---|---|---|---|
| 100 | ... | 2.4 | ... | 2.5 |
| 500 | ... | 4.14 | ... | 4.25 |
| 1000 | ... | 4.63 | ... | 4.6 |
| 2000 | ... | 4.13 | ... | 4.2 |

This will suffice to show that equation (6) will give results sufficiently accurate for all practical purposes.

# ORDINARY GENERAL MEETINGS.

## FIFTH MEETING, 52nd SESSION.

An ordinary general meeting of the Society was held in the Theatre of the Royal Society of Arts, London, on Wednesday, March 7th, 1917, at 8.0 p.m. There was a large attendance of members and guests. The chair was taken by the Right Hon. Lord SYDENHAM, G.C.I.E., F.R.S.

Captain H. J. TIZARD, of the R.F.C., Associate Fellow, read a paper, illustrated by slides, on "Methods of Measuring Aircraft Performances."

On the conclusion of the lecture a discussion followed.

Squadron-Commander BUSTEED: I regret that the Naval Testing Department is not as far advanced as one would like it to be. A good deal of useful work has been done, but the R.N.A.S. and R.F.C. Department had adopted different densities as standards, though these were now the same.

I appreciate the necessity for instruments, but my experience goes to show that the machines instruments were most required for, were single-seaters, and unfortunately, after the pilot had managed to get in, there was very little room for them. They were also a source of trouble in getting tests through quickly; readings taken by pilots had proved very fair.

Lieutenant G. H. MILLAR, R.N.V.R., said that in his opinion it was a pity that the standard atmosphere which had been adopted was a purely empirical one; he would have preferred one based on a given temperature and pressure at sea-level, and a uniform rate of fall of temperature. Some months previously he had calculated such a standard atmosphere, taking as the condition at sea-level a pressure of 760 mm. and a temperature of 15 deg. C., with a fall of 1.5 deg. C. per 1,000ft. rise, and the curve of density against height thus obtained did not differ greatly from that given in this paper, the difference varying between 400 and 700ft. for height from 0 to 20,000ft. By reducing the assumed ground temperature the curves could be brought nearly into coincidence. The advantage of such a standard over the empirical one was that it could be calculated at any time by remembering two constants. He also thought that the unit of density should certainly be the density at zero height for the standard atmosphere adopted. No advantage was gained by using the density for which the speed indicators were initially calibrated, since the instrument had to be calibrated in the machine in any case, and in practice instruments were found to be anything up to 20 per cent. out. With regard to calibration in the machine, calibration at height had the disadvantage that the speed range of the machine was reduced, unless the machine was flown slightly downhill for the higher speeds. He had found that it was best to take four pairs of runs at different speeds over as wide a range of speed as possible, and even with quite rough methods of timing, the four spots usually came very nearly on a straight line. He was inclined to doubt Captain Tizard's statement that the best climbing speed was the same at all heights, although probably little was sacrificed by climbing throughout at the "ceiling" speed. He stated that terms were badly needed for the quantities $\frac{v}{\sqrt{\rho}}$ and $\frac{n}{\sqrt{\rho}}$ where "$v$" was the speed, "$n$" the r.p.m., and "$\rho$" the density. These quantities were of great importance in considering the aerodynamic properties of the machine and propeller (apart from the engine) and the relations between them, and the angle of incidence and angle of ascent or descent were independent of the height or density. He wished to express his admiration for the thoroughness with which the R.F.C. tests were carried out.

Captain GRINSTED: The principle of the methods of testing of aeroplanes and of the reduction of the results to a standard basis as now established and improvements in accuracy of testing can now be made only by improving the instruments by which measurements are made.

Captain Tizard objected to the use of an aneroid as a height-measuring instrument, and preferred to use it simply as a pressure-measuring instrument. Even as such the aneroid is not perfect, and in saying that it measures pressure it is given too good a character. Owing to its lag it does not give a correct measurement of pressure when the rate of change of pressure is at all rapid. I should like to know if Captain Tizard has found difficulty in obtaining instruments sufficiently free from lag for the purpose of accurate aeroplane testing.

The measurement of performance is now confined to tests of speed and climb. There are other things of importance, such as the rate at which the aeroplane can be brought on to a given bank or its direction of flight turned through a given angle which should be measured when comparing performances of aeroplanes. I should like to know if Captain Tizard has considered methods of making such measurements.

Mr. BERTRAM COOPER: I should like to ask Captain Tizard if he could tell us something about the lag of the climbmeter. We have had several "lags" mentioned to-night, but not this "lag," which seems to me to be a pretty serious one.

The action of the instrument depends on the accumulation of pressure in the bottle, which is relieved by the leak. It will be clear, therefore, that the reading will always lag behind the real state of affairs at the spot where it is made, the exact amount depending on how fast the upward or downward journey to that spot was made. For instance, a pilot could stall his machine when seeking his best "climb" and the instrument would still tell him he was climbing when he was, in fact, falling owing to stalling. Moreover, the error here would be aggravated by the "gravity error" on the liquid. This liquid would be relatively lighter owing to the falling, and would consequently tend to show a rate of climb in excess of that actually appropriate to the pressure difference that existed. And this leads me to ask Captain Tizard what is the most serious error in practice, the "lag" error or the "gravity" error? I notice he said that the instrument was not satisfactory near the ground. I take it that is chiefly because of the gravity error and "bumpy" flying, but is not the "lag" error serious at all heights?

Captain FARREN: The methods of measuring aeroplane performance described by Captain Tizard are, as he said, only different in some minor points from those in use at the R.A.F., where they serve both for the testing of new types and for reducing the full-scale experiments on aeroplane resistance, etc., which have been going on there for some time. The methods were, in fact, arrived at to a great extent by discussion between the R.A.F. and the Testing Squadron. The same standards of density are used, and we agree with him generally on the superiority of ordinary instruments and good observers over automatic recording instruments. We have not had so much experience as he has had with single-seaters, which are rather a different problem from two-seaters, demanding much more skill from the pilot, but it seems that even here automatic recorders have disadvantages.

With regard to measuring speeds at heights, Captain Tizard is of the opinion that it is not possible to fly level except by using a statoscope. My experience is that in the case of certain very expert pilots a flight taking as long as ten minutes can sometimes be made, during which the aneroid shows no appreciable movement. (The aneroids used are very high-class instruments, with

20ft. divisions.) I realise that this does not really mean that no height is gained or lost in the test, because every aneroid is known to possess lag. But under these circumstances on the average the height difference between the beginning and end of the run cannot be more than about 100ft. in a length of about 12 miles—corresponding to a slope of 1 in 600 or so, which represents a correction to the speed of well under the error of observation. But undoubtedly the statoscope gives generally much better results. The instrument in use at the R.A.F. is similar in principle to the one shown, but very much smaller, occupying a space 1in. by 1in. by 6ins. approximately. This gives very satisfactory results in use.

With regard to the rate-of-climb meter—which, it may be interesting to know, was christened the "coffeeometer" by the pilots at the R.A.F., on account of the thermos flask used on the first instrument!—this was first shown me by Captain Dobson (then of the Testing Squadron) in July, 1916. A search in the Instrument Stores at the R.A.F. brought to light an exactly similar instrument—made in Germany! It is apparently a standard balloon instrument, but the credit of introducing it into aeroplane testing is due to the Testing Squadron. This instrument again has been much reduced in size, and occupies about the same space as the statoscope, referred to above. In use it suffers from one disadvantage—any vertical acceleration, such as that which occurs as the result of a change in speed, causes the indicating column to move on account of the change in effective gravity. As a result only very gradual changes in speed must be made in searching for the best climbing speed. An attempt has been made to develop a dial indicator (in which the defect would not appear), but without success, on account of the large volume of air enclosed in the diaphragm.

Captain Tizard laid stress on the necessity for very careful work on the part of the pilot. I think too much emphasis cannot be put on this point. We are now emerging from the middle age of aeronautics—when flying was, to the ordinary man, a kind of magic, practised by a sort of superman who daily carried his life in his hands, but nevertheless continued to survive in spite of the apparently rash things he habitually did. To some extent the fear of the passing generation of flyers that flying would become cheapened and commonplace helped to keep alive this idea. They saw their living vanishing. It must be admitted that their fears were justified. It is difficult to estimate aright the value of their work. We are too near to it to see it in its true place. I think we can be sure that history will not be unjust to them or stinting in its acknowledgments. But it is evident that nowadays it is becoming easier and easier to fly—also less risky. The "magic" has gone. In its place we find a new branch of engineering—a new science. For accurate and useful work nowadays skill and nerve are still essential, but to these care, thoroughness, and training in making accurate observations must be added. Everyone who has had anything to do with aeroplane testing knows how it was common talk that A. always got a better climb out of a machine than anyone else. Perhaps he did it by will-power or some other occult practice; anyhow, it was beyond us. Naturally A. gained in many ways, and it cannot be reckoned against him that he did not make any special efforts to dispel the idea. Pilots are human. But the real truth is that A. was possessed of a power of accurate and thorough workmanship, which always, in any kind of work, brings the best results.

Aeroplane testing, as a part of aeroplane designing, demands for satisfactory results the highest training. It occupies no special place by virtue of this—it merely comes into line with the rest of engineering. Now, one can learn to fly in a month—even in England in war time—but an engineer's training requires years. It is evidently necessary, therefore, that engineers—men with scientific training and trained to observe accurately, to criticise fairly, to think logically—should become pilots, in order that the development of aeroplanes may proceed at the rate at which it must proceed if we are to hold that place in the air to which we lay claim—the highest.

I wish to add the following remarks :—

In the years immediately preceding the war aeronautics suffered very much from a lack of full-scale experiments. Money was but grudgingly given, and the foresight of the Government in this matter was not conspicuous. As a nation we are remarkable for our inertia. After the outbreak of war for some time little improvement was evident, but gradually the state of affairs became better. At the present moment we are in a fairly good position, but it is necessary to make provision for "after the war." At the moment experiments are not killed—as they used to be—by lack of money. But after the war the inevitable reaction will almost certainly mean that a partial slump will occur. Money will be scarce and aeronautics will suffer in company with other activities. It is here that the trade must help. They must realise that if they are to build up aeronautics as a branch of engineering they must be prepared to experiment thoroughly. They must provide money and manufacturing facilities for testing and for full-scale experiments. Men will not be lacking. In no branch of engineering have we ever had to want for men—aeronautics has special attractions which will ensure a steady supply of the best. But only if the prospects are sufficiently attractive. A stinting policy here will only result in other countries beating us. It has been our unhappy experience in the past in more than one science to see our brains and our energies wasted owing to lack of encouragement from those who could and should have given it. We have seen other countries gifted with more foresight take our ideas—and our men—and forge ahead. Eventually we have generally managed to regain some of our losses. But in the keener struggle which is to come in every trade we must not go back to the old tactics, or we shall not find Fate so kind to us. It is to be hoped that Captain Tizard's lecture will cause aeroplane manufacturers to see that if they are prepared to treat their productions as other engineers do, to provide for testing and experiment on a liberal scale as is done in every other kind of profession, then they will reap their reward.

Captain TIZARD replied.

Lord SYDENHAM expressed on behalf of those present their indebtedness to Captain Tizard for his interesting and valuable paper.

A vote of thanks was then offered to Lord Sydenham for presiding, and the meeting terminated.

## SIXTH MEETING, 52nd SESSION.

A general meeting of the Society was held on Wednesday, the 7th of March, 1917, at 8.0 p.m., in the Theatre of the Royal Society of Arts, Adelphi, London, W.C. There was a very full attendance of members and friends.

The chair was taken by Prof. A. N. WHITEHEAD, F.R.S., of the Royal College of Science, who briefly introduced the lecturer.

Mr. M. A. S. RIACH read a paper on "The Screw Propeller in Air."

A discussion followed the conclusion of the lecture.

Colonel FULLERTON, R.E. (Ret.): In his paper the author has briefly summarised the theories of the screw propeller enunciated by Rankine and Froude, and has then, by combining the two methods, obtained suitable formulæ for the thrust, etc.

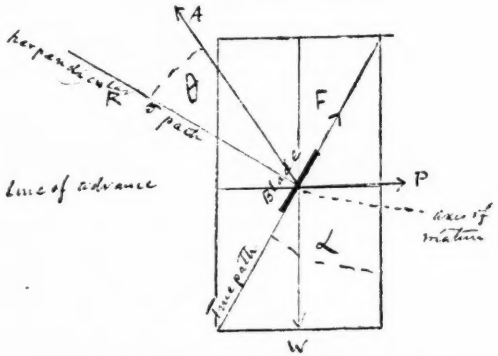
There is, however, I think a simpler method of dealing with this subject, which I explained in my paper on the "Screw Propeller," published in the "R.E. Journal" of December 1st, 1897.

In this I pointed out that the action of the screw propeller is exactly the same as that of the "screw with friction" shown in the ordinary mechanical text books, and that the thrust, power, etc., required can be calculated in precisely the same way.

The accompanying figures, No. 1, "The Screw with Friction," and No. 2, "The Screw Propeller," show this clearly. (Pitch angle diagrams.) Comparing the two, it is clear that $F$ and $R$ combined (Fig. 1) give a force $A$ equivalent to $A$ in Fig. 2.

FIG. 1.

The Screw with Friction.




*As regards No. 1.* Let $v$ = velocity of advance, $v_1$ = velocity of rotation where $v_1 = 2p\pi N$, when $N$ = revolution per sec. and $p$ = distance of $e$ of pressure from axis of screw; then, since $v = v_1 \tan \alpha$

$$\text{Efficiency} = \frac{\text{useful work}}{\text{total work}} = \frac{Pv}{Wv_1}$$

$$= \frac{A \cos(\alpha+\theta)\, v}{A \sin(\alpha+\theta)\, v_1} = \cot(\alpha+\theta) \tan \alpha$$

*As regards No. 2.* Using similar lettering,

$$\text{Efficiency} = \frac{\text{useful work}}{\text{total work}} = \frac{TV}{QV_1}$$

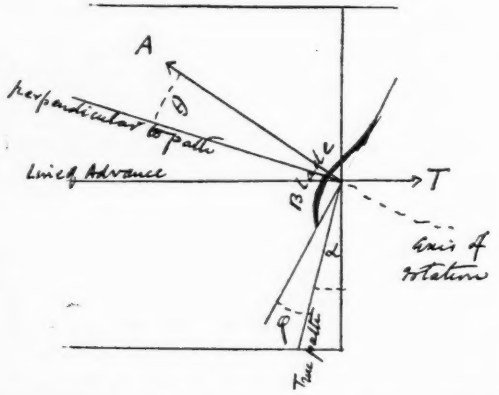
$$= \frac{A \cos(\alpha+\theta)\, V}{A \sin(\alpha+\theta)\, V_1} = \cot(\alpha+\theta) \tan \alpha$$

In either case, once the numerical data are known, all other details can be calculated. As regards the numerical data for any given propeller, the information required is the lift and drag at various velocities of rotation and the position of centre of pressure of the blade. The propeller should be fixed in some suitable testing apparatus and the lift and drag measured automatically at various speeds.

As regards the centre of pressure, this is difficult to measure directly. The best plan is to cut the propeller blade up into portions of 1ft. in width and find

FIG. 2.

Screw Propeller.




the lift, drag and centre of pressure of each separately. The centre of pressure of the whole propeller can then be calculated from the separate results in the usual way.

Once this information is known, the thrust, power, etc., can be calculated in the manner given in the text books for the "screw with friction."

Lieutenant THURSTON remarked that the diameter and pitch ratio of a propeller were of more importance than minor refinements in design. What was wanted was the best compromise giving maximum efficiency at full speed, at the climbing speed, and at various air densities. The theoretically best propeller would probably have so large a diameter that it would be impossible to accommodate it on a machine, moreover it would require heavy and costly reduction gear.

Referring to the "end" effect on the blades of a propeller, it was pointed out that diagrams of the flow of air about a propeller gave average velocities. Although these diagrams showed the air travelling inwards at the edge of the propeller disc, it appears probable that the air in contact with the propeller blade behaved in a similar way to the air engaged by an aeroplane, *i.e.*, the air at the outer edge would slip off giving an "end" effect. As the result of the passage of the propeller blade the air behind is forced away leaving a rarefaction or

partial vacuum into which the surrounding air flows. It is this flow of air which is measured when studying the flow of air about a propeller. Experience has proved that a very fair approximation to the actual conditions of an aeroplane in flight is to assume that the "end" effect can be represented by a parabola over a length equal to the width of the chord.

It was stated that there appears to be no reason why the analogy between the propeller and the aeroplane should not be carried further than has been done heretofore. Each blade sweeps out a spiral which is at a certain distance from the spiral swept out by the other blade or blades. Thus, each blade is in effect running into air which has been disturbed by its own previous passage or by that of the other blades.

A similar condition may be found in the centre planes of a multiplane having a backward stagger, the stagger and gap varying according to the axial speed and revolutions of the propeller. In the case, say, of a triplane with a gap equal to the chord and at a certain inclination; if the lift of the top plane is represented by unity the lift of the middle plane is .65 times that of the top plane and the lift on the bottom plane is .85 times. The effect of the interference on the middle plane can be studied as the backward stagger is increased and as the gap is altered. The middle plane of such a combination, it is thought, supplies an analogy to that obtaining in the case of the propeller, since each blade is engaging air beneath which has been disturbed by the previous passage of itself or another blade, as in the case of the multiplane it has been disturbed by the lower planes and each blade is engaging air above which has been disturbed by the previous passage of itself or another blade and will be further disturbed later by its own passage or that of another blade; as in the case of the multiplane with backward stagger it will be disturbed by the upper planes.

The paper is thought to be very interesting and Mr. Riach is to be congratulated on adding a further contribution to the science of the propeller.

Mr. A. FAGE: Mr. Riach has given us this evening a very interesting and well thought out lecture, and I should like to record my appreciation of the paper.

It is very gratifying to find at the commencement of the paper such a warm reference to the value of model experiments in building-up and formulating a working theory of airscrews. In my opinion we have no *exact* knowledge of the real nature of the air flow around airscrews and of the *exact* manner in which an airscrew does work on the air, and I think a real advance from the present state of our knowledge of airscrews can be made only by a rational combination of sane inspiration and experimental work.

The outstanding feature of the paper is an attempt to co-ordinate the aerofoil and momentum theories, with a view to estimating the inflow velocity of the air along the blade of an airscrew, and so to improve the accuracy of prediction from theoretical data of the performance of the airscrew.

Doubtless to many people here present the lecturer's method of presenting this portion of his subject will be new, but I think I am at liberty to say that before the commencement of the war an investigation of the nature of the inflow velocity of the air into an airscrew was in progress at the National Physical Laboratory, although owing to the urgency of the subsequent war work, we were unable to give much time to this particular research. About the middle of last year, however, a preliminary confidential report giving the results of an investigation of the inflow and outflow velocities of the air in the immediate vicinity of an airscrew, with a view to the improvement in the accuracy of prediction from aerofoil data of the performance of an airscrew, was issued from the Laboratory, and just at present I am engaged in the preparation of other reports.

I will briefly outline our method of calculating the inflow and outflow velocities of the air in the immediate vicinity of an airscrew. The method is essentially a

tentative one and somewhat laborious, in which the absolute inflow velocity, $V_1$, and the absolute outflow velocity, $(V_1 + V_2)$, are so adjusted for any blade element that the thrust, as calculated from a momentum theory, shall be in agreement with the thrust, as calculated from an aerofoil theory in which the inflow velocity of the air is considered, whilst at the same time—and in my opinion this is an important point you should notice—the thrust of the airscrew as calculated from the integration of the thrusts on the blade elements is also in agreement with the thrust of the airscrew as measured experimentally. Thus, for any given working conditions of the airscrew, the correct values of the inflow velocities along the blade and the correct value of the ratio of the absolute velocity of outflow to the absolute velocity of inflow will only be found when the thrusts of the airscrew, (1) as calculated from a momentum theory, (2) as calculated from a new aerofoil theory in which inflow velocity is considered, and (3) as measured experimentally, are all in agreement. Having thus found the inflow velocities along the blade we can then calculate the torque of the airscrew from the aerofoil theory in which inflow of air is considered. This torque, as calculated from the inflow aerofoil theory, is not greatly different from the torque of the airscrew as measured experimentally, and naturally complete agreement is not to be expected because of the suppositions which must be made at the outset of the work. With our new theory we have been able to predict from aerofoil data the actual thrust of an airscrew very closely, and with the use of a small correction fatcor—about 5 per cent.—a close prediction of the torque has also been made.

Unfortunately, Mr. Riach has no experimental evidence to confirm his work, he simply equates the thrust calculated from a momentum theory to the thrust calculated from an aerofoil theory in which inflow of air is considered, and to do this he assumes some value for the ratio of the absolute velocity of outflow to the absolute velocity of inflow. But unless he fortuitously takes the correct value of this ratio, the value of the thrust so calculated will not give the actual experimental thrust of the airscrew, and I think the lecturer is more or less in the position of having to solve one equation containing two unknowns whilst we at the Laboratory have two equations containing the two unknowns.

To make this point quite clear, I will take a numerical illustration from some calculations which were made in the report previously referred to.

For a given speed of rotation of an airscrew and a given speed of translation of the machine, the thrust of the airscrew was calculated to be 300lbs. when the value of the ratio of the absolute velocity of outflow to the absolute velocity of inflow was taken as 1.5, but upon increasing the value of this ratio to 2.5 the thrust as similarly calculated was 345lbs., that is a difference of about 15 per cent. The correct value of this ratio is the one which makes the calculated thrust equal to the experimental thrust.

The theory as explained by Mr. Riach does not, and cannot, calculate the value of this ratio, $(1 + V_2/V_1)$—unless of course the generality of some other variable is tampered with—but at the Laboratory by the utilisation of the experimental performance of an airscrew we are able to directly determine the value of $(1 + V_2/V_1)$.

We have found that the value of the ratio, $(1 + V_2/V_1)$—that is the absolute outflow velocity expressed as a factor of the absolute inflow velocity—depends upon the design of the airscrew, although for any one airscrew the value of $(1 + V_2/V_1)$ is practically constant over the working range of $V/nD$. The mean value of $(1 + V_2/V_1)$ deduced from experiments on different types of airscrews was not greatly different from 2, that is to say, one half of the velocity of the air in the discharged stream is given to the air when it is in front of the airscrew.

Naturally any airscrew theory at the present time has to be based upon many assumptions, but a theory which endeavours to take account of the inflow velocity of the air is certainly a great advance upon the original aerofoil theory, and I

think that the error due to ignoring all unknown irregularities of flow other than that due to the inflow velocity of the air—these irregularities of flow may be those due to the design of the airscrew or may be those inherent in all airscrews—is small compared with the inaccuracy which would result from ignoring inflow velocity.

In any case the inflow and outflow velocity factors, which are determined at the Laboratory from a systematic research upon various types of airscrews, should be extremely valuable so long as the assumptions made in design work are compatible with those made in the researches from which such factors were determined. I believe the designer of airscrews uses large over-all correction factors, but the great disadvantage of these over-all corrections is that they obscure the real nature of the air flow around the the airscrew.

Mr. Riach is probably speculating somewhat when he says that the value of the ratio $(1 + V_2/V_1)$ may be deduced from the ratio of the pressures upon the upper and lower surfaces of a section of the blade. In any case the deductions which he draws from an illustration as to what is occurring when the thrust of the airscrew is zero can certainly not be very conclusive for moderately large values of the thrust.

If I may strike one dissenting note, it would be that Mr. Riach has made the paper look somewhat difficult by the inclusion of masses of straightforward algebra. I think all calculations on airscrews may be simply and rapidly made by graphical methods, which also have the great advantage that one is able to see immediately the function of one quantity to another, and the physical realities of the problem are not hidden amongst tedious calculations. This is however quite a small matter and does not detract from the merit of the subject matter of the paper.

I must admit that I do not like the author's method of correcting for end effects. There is no experimental evidence as to what is really taking place at the tip of an airscrew, especially high speed airscrews where the velocity at the tip of the blade is probably about half the velocity of sound, and I think it is only complicating the matter by applying correction factors which must be very uncertain. When certain that our want of knowledge is very real, I think it is much better to say so definitely in the hypothesis, and there let the matter remain until reliable evidence is forthcoming.

At the end of the paper the author mentioned that the stresses in the blade of an airscrew may be greatly reduced by inclining the tip of the blade slightly forward, and so balancing the bending moment at a section due to the thrust on the outer portion of the blade, by a bending moment called into play by the centrifugal force on this outer part of the blade and the eccentricity between the blade axis and the direction of rotation. This is quite an important point, since by reducing the bending moment at a section, the section may be made thin and consequently aerodynamically efficient. Nearly all the model airscrews designed recently at the N.P.L. have taken advantage of this principle, which, I believe, was in use in Germany before the commencement of the war.

I have now come to the end of my remarks, and in conclusion I again wish to thank Mr. Riach for a very enjoyable paper.

Mr. M. HOLROYD SMITH: Not having had a print of the lecture to carefully study it has been impossible from merely listening to the lecture to form an opinion on the methods of calculation advocated. They have been dealt with at considerable length by previous speakers, experts in mathematics, who did not seem to agree with the lecturer, it would be a presumption on the part of a novice like myself to offer any criticism. I hoped to have heard something new as to the design and construction of screw propellers and came with the intention of giving a description of some of the many experiments I made in conjunction with the Hon. Walter P. Lindsay when we employed new methods of testing screws

of shapes differing considerably from those usually employed and obtained curious and very satisfactory results. The evening is too far advanced for me to do so now, and I should lay myself open to the charge preferred by Lieutenant Lowe (?) against two previous speakers, viz., of giving another lecture.

Mr. TURNBULL: Mr. Chairman, Ladies and Gentlemen,—I do not feel competent to discuss the purely mathematical treatment in Mr. Riach's lecture, but should like to add a few comments in case they be of interest to propeller designers.

Referring to the conclusions at the end of Mr. Riach's paper—(1) I quite agree on the importance of high aspect ratio of blades, provided the "gap" is sufficient to prevent interference. The question of two blades or "multiple blades" is always one that must be governed by the general conditions of each special case, more particularly by the engine revolutions and the "top speed" of the machine. (7) The statement that "a very slight forward rake is usually sufficient to convert the whole stress into a pure tension" is not accurate, as is apparent when we examine the thrust curve of any propeller used on an aeroplane. A typical curve of this kind at first rises and then falls off so that at "top speed" it usually has about one half its initial value. In practice, therefore, the bending stresses are only compensated at one value of the machine speed, but it is safe perhaps to assume that about 50 per cent. of the bending stresses may be compensated for by forward rake and centrifugal force.

As a practical and logical method of design, I have followed the method of Bolas (N.P.L. Report, No. 65, 1911-12) with very excellent results. I have introduced a few modifications of my own, and by careful treatment of the effect of "cowling" have found that no "empirical correction factors" are needed, at least in ordinary cases. I have designed a great many screws by using the method outlined by Bolas, and the actual screw, in every case, has given results in close accord with the calculated results to be expected.

I think, however, that Bolas' article needs revision with special reference to the subjects of cowling and interference, and he makes an assumption concerning "pressure per square foot" that is hardly practical. His formula for this is:—

$$\text{Pressure per sq. ft.} = CBV_t^2$$

in which

$V_t$ = the absolute velocity of an element.
$C$ = a constant depending upon form of section.
$B$ = a constant angle of attack.

Now $C$ is never constant along a practical blade, and it may often be advisable to vary $B$; it is therefore much better to compute the pressure per sq. ft. for *each* section, from the following formula, and plot the pressure per sq. ft. curve accordingly, through the ordinate values obtained:—

$$\text{Press. per sq. ft.} = \frac{\text{lift of sec. per sq. ft.}}{\cos\gamma}$$

$$= \frac{\rho/g \times K_y \text{ (abs.) for sec.} \times V_t^2}{\cos\gamma}$$

$\gamma$ being the gliding angle.

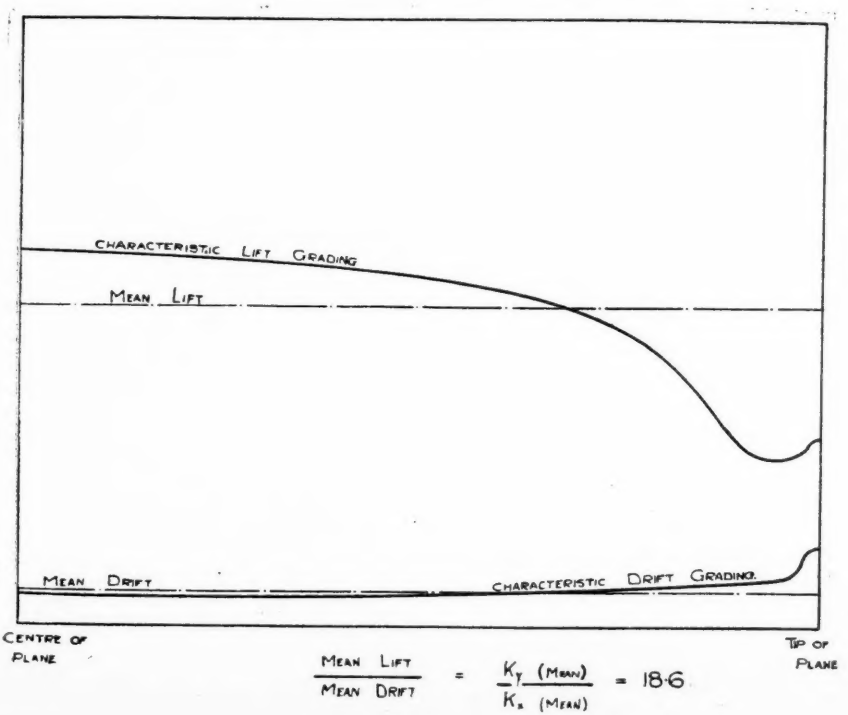
Colonel R. DE VILLAMIL: The statement of the lecturer that there appears to be "no really satisfactory theory of the airscrew," appears to agree with the facts. Why then still retain the name air *screw*? In no way does the action of a propeller in the least resemble that of a screw working in a solid; and, indeed, the

whole paper appears to assimilate its action to that of a blade, or paddle, moving at an angle.

Theories of the "screw propeller" are legion. They are all based on the idea that the thrust varies as "pitch × revolutions"; whilst it is well known that it is possible to design a propeller *without pitch*, which will give a thrust.

Before any considerable improvement in the propeller can be hoped for, it is necessary to find out *how the water* (or air, for it is only a question of difference of density between the two fluids) *gets through the propeller "disc area,"* as it is commonly called. The only suggestion I have ever known put forward was that of Lieutenant Low some years ago, viz., that it was a "source and sink" motion. Sources and sinks are, however, only mathematical fictions—I might almost say toys—which do not, and cannot possibly, exist. The suggestion is not, nevertheless, so very wildly far from the truth as it might appear at first sight. If it be changed into the flow of a fluid *through a circular hole in a thin wall of a tank*, it will not be, I think, far from the truth. We see even the *vena contracta*, the meaning of which no one has properly explained. The solution of this problem, a problem which Poucelet and Lesbros called one of the most important, if not *the* most important, in hydrodynamics, will be found to explain very many curious questions, and will, I feel convinced, lead to a marked improvement in the so-called "screw propeller."

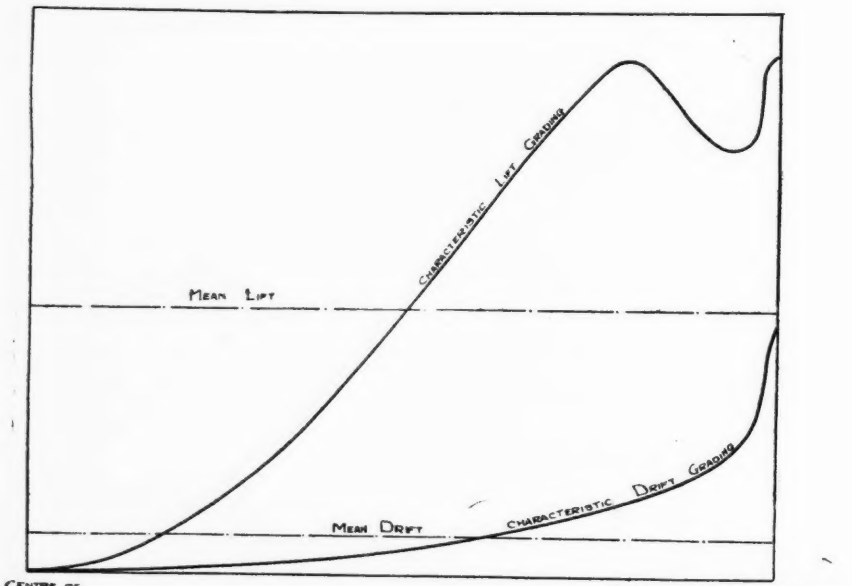
Mr. JOHN D. NORTH: I wish to express my appreciation of Mr. Riach's paper and the valuable contribution he has made to the subject of propeller design. I have been associated with the lecturer for many years, and I have had the pleasure of seeing and discussing the method he now puts forward.




$$\frac{\text{MEAN LIFT}}{\text{MEAN DRIFT}} = \frac{K_y\ (\text{MEAN})}{K_x\ (\text{MEAN})} = 18{\cdot}6$$

I am not altogether in agreement with him in his condemnation of the "old method" of airscrew design, where the inflow was allowed for by an empirical correction factor, and I am not sure that the application of his method will make more efficient propellers. In fact, very excellent propellers are produced by many people without any theory at all. However, that is by the way, as there is no doubt that if the axial velocity in front of and behind the propeller can be accurately determined, a great advance has been made. This brings us to the importance of the direction of flow round the blades which the lecturer assumes to be two-dimensional. A simple presentation of the case is given in the accompanying figure, which shows two half aerofoils, aspect ratio 6, at an angle of attack of 4 degrees, one in a uniform current of air and one rotated about its centre. It will be noticed that in the latter case, analogous to that of an airscrew, the lift-drift ratio is poorer owing to the higher velocity of that portion of the plane where three-dimensional flow takes place. This would seem to indicate that aspect ratio is of great importance and that a suitable correction should be allowed for the difference between the two cases. Of course, the aspect ratio of the average airscrew is in the neighbourhood of 10-12 and the plan form and sections are not uniform. Uniform section and plan are merely given in the figure for simplicity of comparison, as pressure distributions for these are available.

In actual practice the lift-drift of propeller sections seems to be in the neighbourhood of 15-20—that is to say, much higher than one would expect, even allowing for the high velocity. It is therefore not unreasonable to suppose that three-dimensional flow does not take place at the tip of the propeller in such


CHARACTERISTIC LIFT GRADING
MEAN LIFT
MEAN DRIFT
CHARACTERISTIC DRIFT GRADING
CENTRE OF AIRSCREW
TIP OF AIRSCREW
MEAN LIFT / MEAN DRIFT = K1 x r² (MEAN) / K2 x r² (MEAN) = 13·15

RAF. 6. ASPECT RATIO . 6

a way as to produce the lift and drift gradings indicated in the figure, and a definite investigation of this point would be of great value. The measurement of pressure distribution on a propeller is by no means a practicable impossibility, and an account of such an experiment, carried out at the Gottingen Laboratory, is given by Fuhrmann in the April 26th, 1913, issue of the "Zeitschrift fur Flugtechnik und Motorluftschiffart."

With regard to the four-bladed propeller, while this undoubtedly scores on the question of aspect ratio, it is most awkward to transport and expensive to manufacture, so much so that a slight sacrifice of efficiency might well be made in the interests of practical convenience.

Mr. T. W. K. CLARKE: I congratulate Mr. Riach on his further advancement of propeller design. Criticism of his theory I leave to others, but I have with me a couple of slides having considerable bearing on what Mr. Riach said about indraught and its ratio to wake velocity. They are of special interest as he said that there was no experimental verification obtainable.

The description of these and similar experiments appeared in the Journal of the Italian Aeronautical Society for 1912, which that Society kindly sent to me four years ago. The figures represent diagrammatically the result of an exploration of the field of flow in the neighbourhood of an air propeller. The small arrows represent the air velocity in both magnitude and direction.

Using the same notation as Mr. Riach, viz., $V$ the forward velocity of the propeller; $V+V_1$ the relative velocity of the inflow; $V+V_1+V_2$ the relative velocity of the outflow.

The first is shown by the arrows in the outer parts of the field, and the two latter by the arrows close to and on either side of the propeller.

From the diagrams I make out that, in the first case, Fig. 1, *i.e.*, that of a stationary propeller, $V_1/V_2$ varies between .9 and 1.0 except close to the boss and very near the tip; in the case Fig. 2, that of the advancing propeller, $V_1/V_2$ is still closer to 1.

This seems to give a good verification of Froude's theory as quoted and used by Mr. Riach.

There is another method of propeller design, which might be termed the empirical method, to which I should like to take this opportunity of alluding.

All theory and design is primarily based upon experimental results. For propellers usually the experimental results on aerofoils are used, and these, by a process involving necessarily somewhat heavy calculations, are made to give a rational method of designing propellers. But if we start from experiments on types of propellers the calculations can be almost entirely eliminated. The difficulty of course is that the propeller, having more variables such as pitch to diameter ratio, in its design, the number of necessary experiments is multiplied.

If we take a number of geometrically similar propellers then the following formulæ hold:—

$$\text{Thrust } T = Kn^2D^4$$
$$\text{Power H.P.} = \lambda n^3D^5$$
$n$ is here revs. per sec.,
$D$ diam. in feet,

and $K$ and $\lambda$ are constants depending only on the ratio of the circumferential tip velocity to the forward velocity, *i.e.*, on $nD/V$.

The efficiency $\epsilon$ will also depend only on this quantity. Coefficients $K$, $\lambda$, and $\epsilon$ can then be plotted against various values of $nD/V$, or, as I believe is more usual now, against its reciprocal $V/nD$.

We should then have three curves which give thrust, H.P. and efficiency for any propeller of the particular form at any speeds of rotation and travel.

(Two such curves would be sufficient but the three are more convenient.)

Consider in the first place the efficiency curves.

Suppose we have a series of such curves for a set of propellers of similar type but differing in pitch/diameter ratios drawn on one sheet as shown by Fig. 3. Draw the envelope of these.

For clearness the next diagram, Fig. 4, shows only a single curve of the family, but the full curve is the envelope of the whole family series.

Now each point on this envelope is the point of contact of and may be said to correspond to some definite pitch-ratio member of the series. These ratios are marked on the underside of the curve in Fig. 5. Take now a simple case.

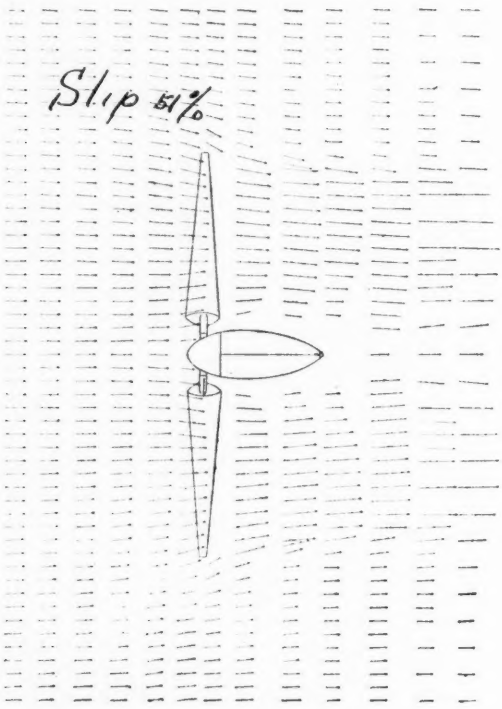



FIG. 1.

Suppose that we require the propeller of this series which will give the maximum efficiency when the diameter $D$ is fixed (possibly determined by constructional consideration); when $n$ the engine revs. are known, and also when $V$ the machine speed at which the best efficiency is required, *i.e.*, its top speed for, say, a speed machine or its best climbing speed or some intermediate speed as the case may be; $V/nD$ is then determined, this then gives a point on the envelope and at this point the proper pitch ratio can be read off. The lower curve in the figure gives the corresponding H.P. coefficients. From the formula $\text{H.P.} = \lambda n^3 D^5$ the H.P. necessary is obtained.

Further, if now there are sufficient experiments to determine the effect of varying blade widths, instead of the single envelope curve, we shall have a series of envelopes each corresponding to a particular blade width ratio. These will themselves have an envelope which is to be taken instead of the original. This new curve, besides determining the best value of the pitch ratio, will then also give the best value of the blade width ratio to be taken in conjunction with it.

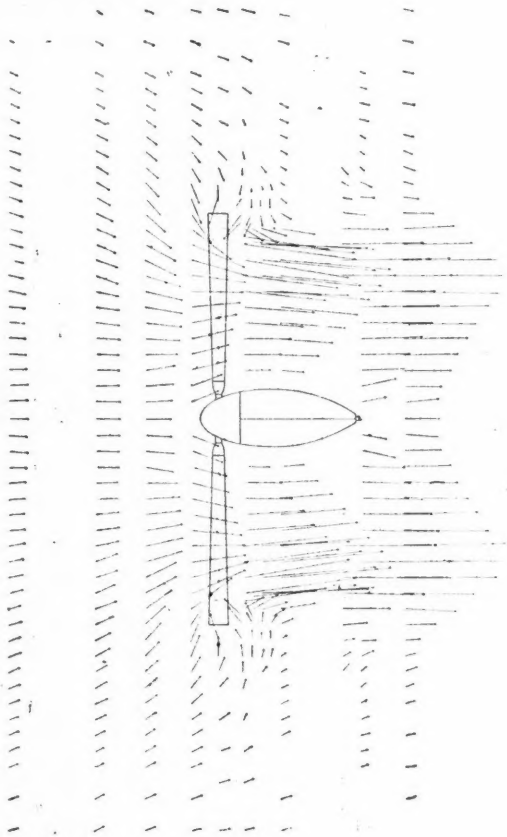


FIG. 2.

We then have a curve as in Fig. 5.

If $n$, $D$ and $V$ are given this can be used at once, as I mentioned, to give the H.P., etc.

But if, as is more usual, H.P., $V$ and $n$ are given, or any combination of the quantities Power, Velocity, Revs., Diam., or $nD/V$, the curve cannot be used directly, but the additional curve shown in Fig. 6 easily deduced from the first enables any combination to be immediately solved.

In this $m$ and $d$ are coefficients involving only $nD/V$ or $\lambda$, which is itself a function of $nD/V$, in fact

$$\left.\begin{aligned} m &= \beta^2\sqrt{\lambda\beta} \\ d &= 1/\beta\sqrt{\lambda\beta} \end{aligned}\right\} \text{where } \beta \text{ stands for } nD/V$$

( (i.) and (ii.) are obtained by eliminating $D$ or $n$ between H.P. $= \lambda n^3 D^5$ and $\beta = nD/V$).

As an example—

If H.P., $V$ and $n$ are given, to find the best value of $D$, etc., use (ii.) in Fig. 6 to give "$m$"; this curve then gives "$d$" and (i.) gives $D$. This curve also gives $nD/V$ and Fig. 5 then gives the efficiency, best pitch/diameter ratio, and width ratio.

So that we see that in any case it is only a matter of a few minutes to obtain the necessary particulars.

Now I have supposed such curves to be completely obtained from experimental data, but this is not necessary although of course by far the better.

They can be derived by calculation from, and form a sequel to, the methods propounded by Mr. Riach, a certain number of actual experimental results being used to give his correcting factors. Then they will give at once the propeller which will best suit the particular requirements.

There is one other point in connection with propellers which has in its time given me, and possibly others equally muddle-headed, some trouble, and that is—given the propeller curves, say H.P. against $V/nD$, also the engine curve, *i.e.*, H.P. against revs., to connect the two so as to know the engine revs. for any particular speed of the machine. The procedure I suggest is this:—

The propeller curve gives $\lambda$ in the formula H.P. $= \lambda N^3 D^5$ against $V/nD$. Multiplying by the constant $D^5$ it therefore gives H.P./$N^3$ against $V/nD$. The engine curve gives H.P. against $N$. From this derive H.P./$N^3$ by dividing the first by $N^3$, at each point we then have curves of H.P./$N^3$ against $N$ and also against $V/ND$, equal ordinates in the two therefore give equal values of $N$ and $V/ND$, *i.e.*, we get a series of corresponding values of $V$ and $N$.

Lieut. A. R. Low: Mr. Riach's paper has elicited two more papers, one from Mr. Fage, and one from Mr. Clarke representing, it may be presumed, the views current at the N.P.L. and the R.A.F. The Air Board designers, it may be added unofficially and impersonally, would claim to be well abreast of these developments some time ago.

The present writer gave an account of Drzewiecki's now well known analogy with the aerofoil in this Journal for April, 1913, and in the subsequent number.

The physical basis of the analogy had not been systematically examined in any previous publication, though Lanchester gave a brief reference in his Aerodynamics 1896 integrating over the blade, in Drzewiecki's manner. For the purpose of reproducing Drzewiecki's results for a particular blade, and similar results for slightly different blades, the indraught velocity was neglected for small slips. But for any subsequent writer to refer to the indraught correction as new is absurd in face of the discussion by the writer, and the references to Soreau on this very point. Boss and tip conditions, interference, aspect ratio were all raised as points requiring further investigation.

The extension of the writer's analysis by Mr. Riach arises directly from the introduction of the indraught correction. It is a thoroughly substantial and valuable piece of work, and gives Mr. Riach a leading place in this branch of technology.

Mr. Riach is not so happy in discussing the physical basis of the method, and seems to paraphrase Mr. Fage's remarks incorrectly. The writer takes Mr. Fage to mean, and agrees with him in meaning, that—

(1) Drzewiecki's method is concerned with the pressures on elements of the blade set up by the deflection of the air passing over the blade. These forces resolved, and integrated over the blade face give the total thrust and torque. The aerofoil analogy of Drzewiecki gives us an indirect experimental method of finding the force on the element of blade.

(2.) The Froude theory considers the change of velocity along streamlines of flow through the airscrew disc. The rate of change of momentum follows at once ; and by integrating through the volume of the stream the thrust and torque are obtained.

(3.) Finally thrust and torque are measured directly.

(1.) Requires a knowledge of the relative velocity of the air approaching each blade element, and the lift and drag co-efficients for aerofoils of similar sections and of high aspect ratio.

(2.) Requires a knowledge of the velocity at every point in the volume of air passing through the airscrew disc along lines of flow.

(3.) Takes no account of the physical mechanism whereby the reactions are produced.

No one should have any doubt whatever about the validity of method 2. It is directly based on the Laws of Motion. The experimental difficulties, as we learn to-night, have been overcome by the N.P.L. and this classical problem definitely placed on an experimental basis.

It will be recalled that the Drzewiecki airfoil analogy was also placed on an experimental basis by Mr. Bramwell's paper in this Journal in 1914, the work being carried on at the N.P.L.

The great advantage of (1) is that the Drzewiecki analogy alone is sufficient to give approximate values of torque and thrust for any given motion when the shape of the blade is known.

It seems to the writer that the airfoil analogy must also be used to correct the changes of velocity, in the Froude method, with the shape of the blade.

From these remarks it appears that though the integrals used in methods (1) and (2) are physically quite different, the methods may both ultimately depend on analogous experiments with airfoils.

Mr. Riach's complaint that he cannot get test data for his airscrews, indicates an entirely wrong state of affairs.

The N.P.L. is after all the *National* Physical Laboratory and *exists* to give technical ideas, methods and results to the nation's engineers, not to any privileged official coterie, and the practical monopoly of air tunnel research it enjoyed for many years is a special reason for generosity. But before imputing blame it would be well to ascertain if Mr. Riach has used due diligence in getting in touch with the right persons.

Mr. M. A. S. RIACH, in replying to the discussion, said: Colonel Fullerton opens his remarks with an inaccurate statement. He says that I have combined the theories of Rankine and Froude, and apparently infers that the paper is based on this. This is not so. I have combined Froude's method of analysis with the ordinary geometrical blade element theory, and the results obtained in this way form the basis of the paper.

Colonel Fullerton gives a method based on the friction screw analogy, but this is open to the same objections as the old aerofoil theory, namely, that his velocity ($v$) is unknown *a priori*, since it obviously involves the inflow velocity of the fluid ahead of the screw.

It is the object of the present paper to obtain reasonable values for this velocity which cannot be obtained on any " screw with friction " analogy such as that quoted by Colonel Fullerton.

I quite agree with Lieutenant Thurston that the pitch ratio is the main criterion of efficiency and that small details of design must always be of less importance in determining the best efficiency values. I am glad to find that he supports the suggestion that the tips of airscrew blades may be treated by analogy with the ends of aerofoil wings. His remark on the multiplane analogy with multibladed airscrews is certainly worth investigation, as such a conception might very well be found to determine an upper limit to the number of blades that may be usefully employed in airscrew design.

I am very gratified that Mr. Fage finds the paper so interesting and suggestive, as he is in a position to judge of the value of such papers as this one much better than the average aeronautical engineer. I am very interested to hear of the methods adopted at the National Physical Laboratory in testing airscrews, and I quite appreciate the fact that such experimental research supplies many of the unknown factors which it is impossible to obtain purely from theoretical considerations.

Mr. Fage's remarks concerning the value of the $(V_2/V_1)$ ratio appear to confirm R. E. Froude's original hypothesis of one half the acceleration taking place in front and one half behind the screw. At the same time I do not necessarily agree with him *a priori* that it is impossible to calculate the value of the $(V_2/V_1)$ ratio from theory alone, although it certainly is not possible by means of the analysis given in the present paper. I am of the opinion that further light may be cast on this point by introducing rotational wake conditions after the method adopted in R. E. Froude's paper of 1892 ("The Theoretical Effect of Race Rotation upon Screw Propeller Efficiency"), and I hope to be able to give the results of such a further investigation at some future date.

Proceeding to Mr. Fage's further remarks, I only put forward for what it was worth the suggestion that the $(V_2/V_1)$ ratio might ultimately be found to be approximately equal to the ratio of the lower and upper pressures on aerofoil surfaces, and the illustration that I gave in the particular case of zero thrust was only intended as an example of what such an assumption might lead to—which in the case quoted appeared to agree fairly well with the facts as determined by experiment.

This also applies to my other suggestion of the analogy of "end" losses on aerofoil wings. I quite agree in general that it is much better not to speculate too much on such points as this when the experimental evidence available is practically nil.

I am glad to find that the method advocated of eliminating bending stresses has been adopted at the N.P.L. The method of course is old, but I think it might be adopted with advantage by many airscrew constructors to obtain better efficiencies.

As regards the method employed in presenting the results of the analysis given in the paper, I cannot quite agree with Mr. Fage when he deprecates the use of what he calls "the straightforward algebra." It seems to me that in a general exposition of such a subject as this it is much more convenient to employ the ordinary mathematical processes, and when, having got all the analysis set out, it becomes necessary to make use of the results so obtained in practical work, it is then quite a simple matter to employ graphical methods of solution wherever necessary.

Moreover, on Mr. Fage's own showing, the values of the thrust given by an airscrew are taken to be those at which the inflow velocity is so adjusted as to make the momentum theory value, the aerofoil theory value, and the experimental value agree with one another.

This is precisely, as Mr. Fage remarks, the process followed in the analysis given in the paper and I really fail to see how a general theory, based on this

admittedly reliable method, could be formulated without the help of the " straightforward algebra " which Mr. Fage considers to be inferior to graphical processes!

The mathematics of the paper are not really at all complicated, although sometimes necessarily rather tedious.

I must again thank Mr. Fage for his appreciative remarks on the paper.

Mr. Turnbull mentions the effect of speed variation, and consequent thrust variation, upon the stresses in a propeller blade.

It is of course perfectly obvious that the centrifugal and bending stresses will only produce a pure tension stress for one value of the flight speed of the aeroplane, and this part of the question was not gone into more fully as it was thought sufficient to briefly refer to this method of reducing stress as an example of what may be done under certain conditions. Mr. Turnbull quotes the N.P.L. report of 1911-12 method of propeller design by Bolas. It seems, however, necessary here to point out that this method is really due to Lanchester and an almost identical process is given in the first volume of his " Aerial Flight."

It is, however, well-known now that this method, if applied without correction factors, inevitably leads to propellers " racing," due to the fact that the inflow velocity is completely ignored on this theory, a fact which Mr. Turnbull apparently has not appreciated.

Colonel de Villamil raises a question of nomenclature, and I would refer him to the Technical Terms Committee of the Society for the use of the word " Airscrew."

I cannot follow him when he says that all theories of the screw propeller are based on the idea that the thrust varies as the product of pitch and revolutions. This does not seem to be the case in modern theories of the screw propeller, and the expressions for thrust given in the paper certainly do not state this. Moreover, if the pitch is zero, the thrust may still be quite considerable.

Mr. Clarke's diagrams are of great interest in that, as he says, they corroborate R. E. Froude's theory.

His illustrations of the experimental method of designing propellers are, I imagine, similar to those employed in the design of marine propellers, and form a sequel to the more theoretical methods given in the paper.

Both methods should, I think, be used in conjunction wherever possible as checks upon each other. Experiment will, I am afraid, always rule theory in the design of propellers until pure mathematicians have evolved a theory which is able to take account of rotational and other effects which at present have to be ignored in the hydrodynamical treatment of this subject.

At the present time pure hydrodynamical theory does not appear to be able to efficiently tackle the screw propeller problem and in consequence the designer has to be content with the semi-empirical theories which are evolved from time to time to meet the requirements of the engineering side of the subject.

I am very glad that Mr. North finds the paper interesting. I agree with him that so far as propeller efficiency is concerned the method of the paper may not be able to do very much in practice, and I quite appreciate the fact that propellers can be, and often are, designed by people with no knowledge of the theoretical portion of the subject, although it is usually experience assisted by good theory that produces the best results in the long run. Mr. North's illustrations of the airflow round aerofoils and airscrew blades are interesting, and the experiments which he mentions carried out at the Gottingen Laboratory are certainly what is required in order to settle this question definitely.

Four-bladed airscrews are necessarily more difficult to transport as well as being more expensive to manufacture. Here again it is a question of compromise between efficiency and practical convenience.

I am glad to find that Lieutenant Low considers the theory developed in the paper as likely to prove of value, but I hope that he does not seriously accuse me of inferring that the necessity for correcting for inflow is new.

What is new is the theory given in the paper by which the quantitative amount of such correction can be estimated.

I do not quite follow his further remarks regarding the references made in the beginning of the paper. R. E. Froude's theory does not pretend to be capable of an exact physical interpretation, his "actuator" being an ideal conception. This being so it would have been incorrect to assume that the method given in the paper was based on conceptions which could be regarded as possessing an exact physical basis.

Prof. WHITEHEAD, F.R.S., from the chair, expressed the thanks of the Society to Mr. Riach for his valuable lecture.

The Chairman being cordially thanked for his services in presiding, the proceedings terminated.

## SEVENTH MEETING, 52nd SESSION.

An ordinary general meeting of the Society was held on Wednesday, April 4th, 1917, at 8.0 p.m., in the Theatre of the Royal Society of Arts, London. Colonel Sir CAPEL HOLDEN, K.C.B., M.I.E.E., F.R.S., R.A., presided over a very large attendance.

Mr. A. P. YOUNG, A.M.I.E.E., A.I.A.E., read a paper on "The High-Tension Magneto, with Special Reference to the Ignition of Aeroplane Engines," illustrated by lantern slides.

A discussion followed the reading of the lecture.

Lieutenant-Commander BRISTOW: Mr. Young is to be congratulated upon a paper which is in effect almost a treatise on almost everything that appertains to the theory and practice of magnetos.

There are many points which could be discussed with great advantage, but unfortunately the time at our disposal does not admit this. I feel that justice could not be done unless Mr. Young, with a few other searchers after truth, were shut up together for a week, which would be all too short in which to debate the many points raised.

There appears to me, however, to be one aspect of the question which has not been touched upon and that is the relation generally of the magneto to an aircraft engine. At the present time, it seems that the whole design of the magneto and its method of fixing to the engine is but a development of gas engine and motor car engine practice, and there is little doubt in my mind that if we could start free from all tradition and with time in which to prepare an ideal equipment, the ignition of aircraft engines would be accomplished in a much more efficient manner.

By efficiency I mean not only the number of sparks per second per pound of weight but also efficiency as regards providing some method of attachment which would not only reduce engine weight but eliminate many of the troubles which arise from the manner in which magnetos are at present fitted.

At present the magneto makers work to a height of spindle with their own gauges and the engine makers prepare their base accordingly from another set of gauges and when the tolerances of both sides are taken into consideration, the result in many cases is an out of alignment which has an extremely detrimental effect on the magneto.

In addition to the height difficulty, there is also the question of axial alignment, and as the couplings which are generally provided do little or nothing towards compensating for these errors, the results are extremely unsatisfactory.

When it is considered also that replacements in Service have to be made in all sorts of out of the way places, very often under extremely difficult circumstances, it is obvious that in the aggregate a great amount of damage must be caused by the continuance of this method.

With the present design of magneto, a base is of course necessary, but were the machine redesigned altogether, as it can be, the equipment could be much improved.

I would suggest in the first place, that the electrical design of the machine be improved with a view to eliminating the iron losses in the circuit and thus making it possible to employ a much smaller armature and magnet. Further, steps could be taken to reduce the length of the machine and the weight, so that the end plate of the magneto could be provided with a flange which should push in to the crankcase and be bolted thereon through lugs on the end shield. If the end plate were provided with a decently wide spigot, which should be arranged concentrically with the centre of the armature spindle, and the hole in the crank-

case were bored central with the drive, the machine would be self-aligning and would be easily replaceable with any other magneto of the same type. This would enable the base plates on the engine and their supporting webs to be entirely dispensed with, and as the magnetos would weigh only about 8 to 9lbs. each, a very considerable reduction in the total weight would be possible and a number of possible points of breakdown would be eliminated.

In addition the question of advancing and retarding the ignition seems to demand some attention. It is agreed by all engine designers that it should be possible to considerably advance the ignition on engines running at high altitudes and this necessity appears fairly obvious. Up to the present it has been customary to look to the magneto maker to provide this advance and this considerably hampers the magneto designer and is perhaps calling upon him to do something which can be much more easily done by the engine maker.

On an engine with a number of magnetos there is a certain amount of difficulty in securing synchronisation of the magnetos at all points in the range and in addition the weight of the control gear is quite considerable. This could probably all be avoided by incorporating with the engine magneto drive one spiral coupling controlled by a single lever which would advance and retard all the magnetos simultaneously and would enable a fixed ignition magneto to be used. This would in some cases simplify the design and construction of the magneto very considerably and eliminate many difficulties in working which are introduced through incorporating this advance and retard mechanism on the magneto itself.

I feel that the ignition for aero engines has not been sufficiently thought out and feel confident that the near future will produce a very considerable improvement in the present methods.

I should like to again express my appreciation of the paper and the thanks which are due to Mr. Young for the compilation of such a large amount of information on this very important subject.

Lieutenant FAIRBROTHER: In the first place, I should like to congratulate the author on his comprehensive, interesting and most useful paper given to us at a very opportune moment, dealing with a most important and vital subject concerning the ultimate efficiency of the internal combustion engine.

Without wishing to appear presumptuous, I should like to make a few remarks in the way of constructive criticism.

On page 3, part 5, the author is of the opinion that the magnetic field should instantly collapse at "break," and that to attain this object careful attention should be given to the magnetic circuit through which the magnetic field passes. Now, it appears to me from an examination of contemporary designs, that insufficient attention is given to this point. In the inductor type magnetos, the magnetic flux passes mainly through laminations, but the shape of this circuit and the way the joints are made are instances of bad design and there is considerable room for improvement in this direction.

On page 4 reference is made to the relation between spark energy output and magnet strength.

I am not altogether in agreement with the author's views on this point, for recent experiment has shown that the energy output is mainly dependent on coercive field and is approximately $\alpha$ to the $H^2$. It does appear that the product coercive force and remanance might well be rather misleading in character; the conditions in the magneto are very similar to dynamo design. To get the requisite effect, it is necessary that you should have a certain inductance, and to maintain that effect under working conditions, it is necessary that you should have a certain magnet-motive-force. The magnets will be unsatisfactory if the magnet-motive-force is too small. (Magnet-motive-force is made up of coercive force and length, but are not interchangeable when $H$ is low.)

The remanence is a relatively unimportant matter as long as the inductance is maintained. Therefore, that the value of the magnet is proportionate to $(B \times H^2)$, or any function of these would appear to be deceptive. What is required is a sufficient number of lines and the necessary magnet-motive-force to resist armature reaction.

While the English steel makers have succeeded in producing a very excellent magnet from a magnet standpoint, there is undoubtedly room for improvement in the methods of manufacture, and it is anticipated that such tolerances as 16/1,000in. between the pole faces will very soon be considerably reduced.

Referring to the designs of cams, there is a great tendency to favour separate cams fixed in a housing in preference to one solid cam.

In the former case, the cams can easily be adjusted to attain efficient synchronisation where two magnetos are used concurrently, whereas in the latter case it is necessary to work to very fine limits in the manufacture of the cam. The angular error between two cams displaced at 180° should not exceed .50°.

Referring to the fixing of magnetos, the French engine builders have now decided—in order to secure correct alignment—to drill (2) centralising holes in the base of the magneto 64 m.m. centres on the centre line and to locate the magneto on the engine bracket by means of pins in the engine magneto platform.

**Brush.**

In my opinion more attention should be given to a successful combination of carbon brush and distributor material and that it is no use considering each composition separately.

Experiments have shown that a carbon brush containing little or no graphite is the most suitable. Careful attention should also be given to brush pressure. An initial pressure of 1½ to 2½ 1 sq. in. is quite satisfactory.

**Engine Starting.**

Method C is undoubtedly at the present the most favoured method, but it is not proving very satisfactory for high powered engines. It would appear that some indicator is necessary to show the most advantageous position of the crankshaft for this method of starting.

I do not think that any definite angle can be given for the trailer point, as the final position is governed by so many factors.

**Magneto Testing.**

It would appear that there is now sufficient data available to enable a standard and universal test to be formulated on a similar basis to engine testing. In the first place a definite type of calibrated gap should be standardised; at present there are at least three forms of gaps used—points, balls, concentric gap. What is required of a magneto is a certain number of efficient sparks per minute as distinct from R.P.M.

For example, a sleeve type magneto producing four sparks per rev. will run at engine speed on an 8-cylinder engine, whereas a rotating armature magnet giving two sparks per rev. will run twice engine speed. Obviously, it is quite unfair to test these magnets at the same speed.

Therefore, I suggest that magneto test should be based on the number of sparks produced per min. and not on magneto speed in R.P.M. It is very desirable to include in the standard test commercial methods of measuring energy per spark and primary voltage.

**Variable Timing.**

It appears to me that the English manufacturers of magnetos had followed the Bosch Co. like sheep on this point.

The method of obtaining variable ignition as adopted by the Bosch Co. would seem to be fundamentally wrong. For as the power of the spark is proportional and increases with the speed, it does appear that the gap between the armature core and pole shoes should increase with the speed. The present method is to increase this gap at low speed and decrease it at high speed, which is entirely opposed to what should be, as indicated by the result of all experiments.

**Battery *v.* Magneto.**

In deciding the relative importance and advantages of either battery or magneto ignition, one should draw a line of demarcation between road vehicles and aeroplanes.

The only advantages that appear to accrue from battery ignition are:—

Cheapness of installation.
Easiness of starting.
Efficiency at very low crankshaft speeds.

None of these points apply in the same degree to aeroplane engines. From a brief summary of the whole situation, it would appear that:—

(1) A spark of constant head value and short duration as obtained from an open circuit battery system must entirely rely on spark advance for meeting varying engine conditions.

(2) A spark which grows weaker (in closed-circuit battery system) as the engine speed increases, is wrong in theory and practice.

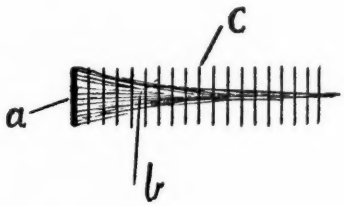
(3) A spark that increases in intensity as the speed increases conforms directly to the requirements of an engine. This kind of spark is obtained from a magneto.

Mr. J. D. MORGAN: For the second time Mr. A. P. Young has placed those interested in internal combustion engines under an obligation to him for a lucid exposition of a difficult subject. Naturally, when the phenomena are so complex as they are in the high tension magneto and especially in the ignition effects produced by that apparatus, different investigators easily arrive at different interpretations. There can therefore be no suspicion of disparagement in my present offer of an alternative view of the process by which a spark produces ignition. In section 5 Mr. Young says "the maximum value (of the secondary voltage) reached would seem to have some influence on the power of the spark to produce ignition," and later, "I rather suspect that the maximum value of the secondary voltage is a controlling factor (in ignition) and it is even within the bounds of possibility that the ionisation occurring as a result of the sudden application of an enormous voltage during an extremely small interval of time may in itself be sufficient to initiate the explosion."

This statement implies that high voltage is essential to ignition, and a direct consequence is that low voltage (or low tension) sparks are less effective in producing ignition than high tension sparks. But this is contrary to experience. Broadly speaking, it is the energy cotent of a spark which determines its incendivity (or ability to produce ignition). It is probable that the rate at which the energy is discharged through the spark also has an effect. The energy can be so far reduced that notwithstanding high voltage, ignition of a given gas is not followed by the passage of the spark. Further, any ionisation which might be attributable to the high voltage is so slow in its action as to be of no practical value as the only source of ignition in an engine. The question therefore arises, What is the function of the high voltage to which Mr. Young refers? I venture to suggest the following explanation.

When a high tension spark is examined in a rotating mirror it has among other effects the characteristics illustrated in the accompanying sketch.

(*a*) is an intensely bright blue line which forms the beginning of the spark. This is followed by a flame (*b*) on which is superimposed a group of faint purple lines (*c*) due to oscillation in primary. (*a*) represents only a portion of the energy of the spark. Usually it is sufficient in itself to cause ignition. But it can be made so attenuated that by itself it cannot produce ignition. The part (*a*) is obviously directly associated with the secondary and is probably due to the discharge of the capacity current of the secondary. In other words, it is the discharge of the current accumulated in the secondary and its external circuit during the




development of the pressure necessary to break down the gap. It is a fact easily capable of experimental verification that the ability of a given apparatus to produce a spark across a given gap depends upon the speed at which the part (*a*) can be developed. As is well known, plugs and plug conditions vary considerably. An apparatus which is relatively slow in its action may fail to project a spark across a plug when a quicker apparatus easily succeeds. I have therefore come to regard the part (*a*) as a pioneer spark and attribute to it mainly the function of breaking down the gap and thereby forming an easy path for the remainder of the energy of the spark. It is quite correct to say with Mr. Young that for the inception of a spark by high tension ignition apparatus a high voltage developed at a great speed is essential, but in my opinion the purpose of this in practice is rather to cope with adverse plug conditions than to contribute directly to the ignition of the gas.

In the penultimate paragraph of Appendix II. Mr. Young states "Experience has shown that a spark which increases its intensity with the speed" automatically speeds up "the propagation of the explosive wave." Mr. Young may be right. I should very much like to know the evidence for his statement. In my own investigations I have come across nothing which would enable me to endorse his opinion. An exhaustive laboratory investigation of this point has led me to the conclusion that if a spark is sufficient to produce ignition increase of the spark has no effect upon the rate of development of the explosion. Such engine tests as I am acquainted with confirm the conclusion.

Dr. G. E. Bairsto: Mr. Young's paper touches on some highly controversial matters, and I think that several statements in it are open to question.

First, as to the use of the "spark-gap" type of distributor. I cannot agree with his statement that it has proved "thoroughly reliable and satisfactory." My experience is that the clearance mentioned, about 0.01in., is rarely sufficient to prevent the points rubbing on the brass segments when the magneto warms up. This rubbing eventually produces a fine deposit of brass dust on the track, and then misfiring occurs. If, however, the gap is increased much more than 0.01in., there is a marked decrease in the available length of spark that the magneto will give, and this decrease is much more marked at low speeds. The

extent to which this trouble occurs depends upon the particular machine, and it is principally due to the combination of the following causes:—

Radial expansion of the rotating distributor when warm.
Eccentricity between the rotating distributor and the track.
Side play of the distributor box in its housing.

I quite appreciate the advantage of feeding the spark plug via a small gap in the distributor, but I consider that the design of the distributor and its casing require considerable modification before the spark-gap type of distributor can be used with complete success.

I should be very glad if the author would state what experiments form the basis for his statement on page 4—that the active core flux in a magneto is greatly dependent on the product of the remanence and the coercive force. He also adds that the spark energy output being proportional to the square of the armature core flux must, in consequence, be dependent on this product. I fail to see how the armature flux and the spark energy can be functions of the coercive force, and it seems to me to be an unscientific method to use the product of the coercive force and the remanence as a basis for forming a criterion of the suitability of a magnet for magneto purposes. It would be preferable to consider each separately, the remanence to have a minimum value, in order to obtain sufficient flux and spark energy, and the coercive force should be as high as possible so that demagnetisation may be a minimum.

The author states on page 10 that a carbon brush which takes a high polish is desirable. This may be true up to a point, but if the polish reaches such a degree that the surface skin becomes "glazed," a softer carbon would be desirable. "Glazing" means that the carbon is too hard, and it entails brass dust upon the track, and consequent misfiring. A "glazed" surface is also instrumental in decreasing the spark length, as the skin has a high ohmic resistance.

The problem of variable timing has been discussed in the paper, and the special difficulties that this entails have been pointed out, but no mention has been made of the simple plan adopted in the Dixie magneto. Here variable timing is secured by rotating the pole shoes together with the armature core round the rotating inductor shaft. By this means the primary current at break can always be kept constant at all speeds.

Mention has been made of the loss of flux in the armature due to the use of steel straps completely embracing the magneto. Some tests which I carried out some time ago on three machines of the B.T.H. A6 and A8 types, showed even greater losses, amounting in each case to 12%, corresponding to a loss of spark energy of 26%.

There seems to be a slight discrepancy on page 9 (see fourth line). Apparently a printer's error has turned .015 into 0.15.

The author has made a brief reference to the process of ignition, and I agree with his view thereon. The general trend of all the evidence now available seems to be that the *main* factor governing efficient ignition is the rate of growth of the electromotive force across the spark plug. It may be of interest to mention that a research into the process of the electrical ignition of a charge in an engine cylinder has already been initiated at the Royal Aircraft Factory, and it is hoped that more light will very soon be thrown on the obscure points which surround this very important subject.

Mr. A. BROOKS: Mr. Young states that "At the outbreak of the war practically no permanent magnets were produced in this country, and that this industry was entirely in the hands of the Germans."

Whilst it is quite true that most of the permanent magnets were purchased from Germany, the firm with whom I am associated have manufactured permanent magnets for telephone generators and other apparatus to the extent of approxi-

mately 120,000 per annum since 1905. Throughout this period the steel has been made by a Sheffield steel manufacturer to our specification.

Of course, before the products reached the desired standard, a considerable amount of investigation had to be carried out with regard to chemical composition, processes of manufacture, and hardening, etc., but as a result of this work we obtained magnets with magnetic qualities equal to those at present being supplied by the Sheffield manufacturers for magnetos.

The precise effect of the various magnetic characteristics of a magnet upon the output of a magneto are at present rather indefinite. There is no doubt that the coercive force and remanence are determining features, but I do not consider that the product of these terms is the criterion of the value of a magnet. With large air gaps and high armature speeds the C.F. is probably of greater importance, whereas with small air gaps and low speeds the remanence has most effect. The shape of the descending knee of the B.H. loop between the +B. axis and the negative H. axis is really the chief criterion of a magnet steel, but it is impracticable commercially to determine this portion of curve for each magnet tested, and hence we can only take the values of remanence and coercive force in conjunction with their product.

The C.F. is certainly very important, as it not only largely determines the flux in the armature core, but also, as our investigations at Beeston have shown, it is a vital feature in the ageing loss given by the magnet, *i.e.*, the reduction in flux with time. This reduction is appreciable during the first running of the magneto owing to the armature reaction effects, but later develops to a fairly steady depreciation whether the magneto is run or not.

I do not consider that the value, 55 for the minimum coercive force, as set by Mr. Young, is high enough, as this should not be less than 58, and I certainly do not consider that the product only should be measured, as magnets can be obtained from the same supplier with widely differing values of C.F. and remanence. In some cases a low C.F. may be given with a high remanence, and in others a high C.F. with low remanence, the latter probably being the better all round the magnet, even if the same product be given.

Then again, the values of these terms are dependent very largely upon how they are measured. As a matter of fact, we have proved that however carefully the magnets are hardened the C.F. will vary at different points of the magnet, and also the flux corresponding to any magnetising force will vary throughout the length of the magnet, being very different near the drilled holes for instance. The measurement of the magnetic characteristics of permanent magnets is very difficult, and widely varying results can easily be obtained by different methods of measurement. This point is at present a source of considerable worry to the English steel manufacturers, chiefly owing to the question of rejections of magnets by magneto manufacturers.

The drop in output given by one of the Bosch latest type magnetos, as mentioned later in the paper, is, I think, largely due to the low coercive force of the magnets, because I understand that these were low compared with earlier Bosch or present British magnets.

In regard to grinding limits, I do not agree that the trouble with grinding to fine limits is due primarily to springing as there should be no difficulty in designing grinding jigs to overcome this. The trouble appears to be due to the alteration in shape of the magnet by the quenching during hardening, owing to varying stresses set up by the sudden change in temperature, and also to internal stresses existent before the heating process, due to being, etc. Grinding to finer limits could be done, but the cost would increase out of all proportion, as it is not necessary to go beyond the .2 m.m. limits set.

From the curves it appears that the polar inductor type of magneto gives a very much greater loss in energy on high speeds than does the rotating armature

type, even when the latter is fitted with the usual solid pole pieces. This appears to be a characteristic of the polar inductor type, and very considerable trouble has been experienced in this country owing to misfiring on high speeds with a well known American magneto built on this principle, and fitted to motor cycle engines. The values of the energy output for the complete range of speeds also appear to be quite appreciably less than those given by the general British or German rotating armature magnetos.

I should be glad to know if, considering the high primary voltage as stated in (4), the assumed high secondary voltage has been actually measured. If an abnormally high secondary voltage is given, would not this tend to give increased troubles with insulation difficulties, both in the magneto itself and in the spark plugs? It is easier to overcome the said difficulties, of course, with the stationary polar inductor type coils.

It is interesting to note that the loss of energy output due to the use of steel straps is as much as 12%. I would like to know if this figure were obtained with the magneto fitted to the usual aluminium crank case and by the straps only. If so, the question is important, especially if one considers the fact that some car manufacturers fit mild steel packing pieces to the bottom of the magneto, and frequently use steel base fixing screws or dowel pins in addition to the fixing strap, also many cycle magnetos are fitted on steel mounting plates. Then again the loss would probably be greater with the small car magnetos having a shaft height of only 38 m.m. instead of the 45 m.m. of the large magnetos.

The stated inherent defect of an H.T. magneto, *i.e.*, that it gives less intensity at the retard position on low speeds, can be overcome by the use of special auxiliary pole pieces separated from the main pole pieces by non-magnetic material, or by specially shaping the tips of the pole pieces or armature core. In the magneto now being manufactured at Beeston the polar system is so designed that at low speeds the intensity of spark is even greater with the lever in the retard position than in the advance, whereas on speeds above 500 r.p.m. the intensity is practically uniform throughout the whole range of timing. The spark intensity of the Bosch ZF4, Za4 and Za2 magnetos, drops off to less than 50% in the retard position on low speeds.

This feature is very important as regards the efficiency of magnetos because, unless the engine is fitted with some auxiliary starting device, starting difficulties may occur with magnetos as ordinarily constructed, owing to the low efficiency in the retard position at cranking speeds. This is especially important to the car owner during the days of poor petrol and inferior substitutes.

At high speeds, assuming anything like good carburation, the slight dropping off in efficiency in the advance position is negligible as the output is comparatively great, and it is also important to bear in mind that with ignition advanced the spark occurs when the compression in the cylinder is very considerably less than the maximum.

Lieutenant J. S. IRVING: I should like to congratulate Mr. Young on his excellent paper, and also upon his statement that certain developments now taking place will prove to be vastly superior to anything produced by Germany before the war, and trust that this statement will also apply to any German product after the war.

I was very interested to see the spark gap distributor put into general use. During the early months of the war this was discussed with Mr. Young, owing to the difficulties then being experienced with insulating material. My suggestion then was to use moulded glass. If this could be a manufacturing proposition, it would have many advantages over the present insulating material. Another advantage of the spark gap type of distributor is that it introduces a spark gap in the secondary circuit, which compels the voltage to rise to approximately 1,000

volts before flow commences, and in effect consists of an external spark gap, the advantages of which are known in every engine test house in the country. The chief disadvantage of the present design of spark gap distributor is the fine limits which it imposes on the assembly of the high tension distributor mechanism, and the difficulty of setting the steel brush to the required limit.

I should like to mention that the Watford Company were producing magnetos in small numbers in the latter part of 1914, which were at least equal to the Bosch product. In recognition of this success, the firm were overloaded with a huge order for small oil bottles for rifles.

The polar induction type of magneto was on the market somewhere about 1909, under the name of the "Coates Generator," which gave six alternating current waves per revolution of the inductor. This outfit was fitted to a Daimler car at this period, and, I believe, ran quite well, but, as a separate induction coil was used, was rather bulky.

Mr. Young makes a statement that the B.T.H. re-magnetise after a 20 hours' test. I assume that this would be a precaution, and that it is not considered necessary to re-magnetise after 20 hours' running.

I am also pleased to see that a phosphor bronze bush is now being used on the contact breaker lever. This bush up to the present has always been fibre, and has, undoubtedly, caused more trouble than any other detail of the magneto. The aeroplanes with the Expeditionary Force have to be kept in more or less exposed conditions and constant trouble has been experienced with the seizing of the contact breaker lever. As many as 50% of the aeroplanes in the squadron have been found unserviceable from this reason when lined up for the early morning reconnaissance. To prevent this trouble it has been necessary to ream out the fibre bushes until they are a very slack fit on the fulcrum pin. I have carried out some tests, using phosphor bronze bushes for this purpose, and I have found them to be perfectly satisfactory. One test consisted of running two magnetos for 150 hours without attention. The phosphor bronze bushes were allowed a clearance of .004 m.m. The fulcrum pin was slightly greased at the original assembly, and no further lubrication was used throughout the test. At the end of the test the bushes were in excellent condition, still greasy, and the wear was inappreciable.

Mr. A. P. YOUNG: I am glad that Lieut.-Commander Bristow has opened the discussion this evening, because in his official capacity he has watched the growth of a magneto industry since the outbreak of war, and has done much to stimulate and encourage this development by fostering a spirit of co-operation between those engaged on this important work. The British magneto industry as a whole is indebted to him for the efforts he has put forward in this direction.

I am in agreement with his remarks *re* the alignment of magneto with respect to the engine. I agree also that there is considerable room for improvement in respect of the method of mounting the magneto and the design of the flexible couple used for transmitting the drive. The method of mounting referred to is quite simple and sound, although it is only applicable to light weight machines. At the present time it is applied to certain single cylinder machines and the type ADs magneto, illustrated in Fig. 9, is a good example. The tendency in the future will undoubtedly be in the direction of reducing the overall size and weight, so that the method of mounting favoured by Commander Bristow may eventually be incorporated in multi-cylinder magnetos for six, eight and twelve cylinders.

I am entirely in agreement with Commander Bristow's remarks concerning the provision of suitable means for advancing and retarding the ignition, and have briefly dealt with this question under the heading, "The Problem of Variable Timing," on page 203.

Lieut. Fairbrother states that there is room for considerable improvement in the design of the magnetic circuit used in inductor type magnetos, but does not clearly indicate how such improvement is to be effected. Admittedly, the magnetic circuit in an inductor type magneto possesses considerable reluctance and gives rise to a large leakage flux, in consequence of which the active flux in the armature core is small as compared with a rotating armature type magneto. Notwithstanding this, however, the fact that there are no solid iron masses in the armature core circuit likely to produce damping of the flux change at " break," more than counterbalances this apparent defect.

I do not quite follow Lieut. Fairbrother's argument concerning the function of a magneto magnet and the relation between its magnetic characteristics and the energy output of the magneto. The only point I wished to make in my paper was that the active armature core flux (N)—considering a given design of magneto—is *dependent to some extent on the magnet strength* of the magnet used. It is easy to see that the energy output of a magneto is proportional to $N^2$, so it is obviously desirable to use magnets having as high a *strength* as possible—that is, magnets with high coercive force and high remanence.

I agree with Lieut. Fairbrother that Sheffield magnet manufacturers should endeavour to improve upon a tolerance of .016″ for the dimension between poles, and believe that much finer limits are being worked to at the present time.

Lieut. Fairbrother appears to have somewhat unorthodox views on the question of testing. I agree that—in theory—the efficiency of a magneto might be ascertained by determining the maximum and minimum number of sparks per minute that the machine is capable of producing between electrodes of some standard design, set to give a definite gap. In practice, however, this method of viewing the problem, particularly from the point of view of the low speed characteristics of the machine, does not work out as it should do. For example, suppose we have two eight-cylinder magnetos, one giving two sparks per revolution and the other four sparks per revolution. If the maximum engine speed is 2,000 r.p.m. then the maximum number of sparks per minute required = 8,000. These are given by the two spark machine, running at 4,000 r.p.m., and the four spark machine at 2,000 r.p.m. The former machine would have to be specially constructed to give the desired output, whilst no difficulty would be experienced with the latter.

Considering the low speed sparking, the two spark machine could easily be designed to give a minimum number of sparks per minute equal to 200. It has not yet been possible to design a four spark machine to give such a low minimum and at present the figure is approximately *twice* this. We see, therefore, that whilst the proposed method of testing is logical from the engine standpoint it is really unworkable because it does not take into account inherent differences in the operation characteristics of the various types of magnetos now being used.

Lieut. Fairbrother's criticism of the method of varying the timing adopted by Bosch and used at the present time by English manufacturers is hardly reasonable. It is fully appreciated that for the best results, the air gap between the edge of the armature core and the pole tip should be increased as the speed is raised, and vice-versa. It is simply unfortunate that the method of varying the timing by rocking the cam-cage produces the reverse of this. On the other hand, it is a very simple and obvious way of achieving the desired result and it is for that reason that English manufacturers have adopted it—not because of its inherent weakness from an electrical standpoint.

Mr. Morgan's contribution is of very great interest to me, and I am glad that he is able, in the light of his own very extensive investigations, to give indirect support to the view I have put forward. I would infer that the part of the spark (*a*) is controlled by the rate at which the voltage rises across the

sparking electrodes, and that other things being equal, a more rapid development of this part corresponding to a higher "maximum voltage" in Fig. 6 would produce better results from the ignition standpoint. Dr. Bairsto, I am pleased to learn, corroborates this view, and it is gratifying to know that a research into the process of electrical ignition has already been initiated at the Royal Aircraft Factory. I personally feel that a careful and elaborate research of this kind will reveal information of great value to the magneto industry—and it may have the effect of ultimately revolutionising magneto design. I hope, therefore, that when this research is completed the results obtained will be given the widest publicity and not filed away in some Government Departmental docket.

Dr. Bairsto's reference to the use of a spark gap distributor is of great interest, and he will no doubt be interested to learn that as a result of a recent investigation made by him we have increased slightly the clearance between the rotating metal brush and the brush track of the distributor, making the minimum limit .010" and the maximum limit .015". It is anticipated that with the slightly larger clearance the possibility of trouble due to rubbing will be entirely eliminated.

Experiment has shown that the active armature core flux in a magneto of given design is dependent, within limits, on the product of the remanence and coercive force of the magnets used, or upon what I have termed the "strength" of the magnets. That is, if we consider magnets having a minimum coercive force of 55 and a product of remanence and coercive force ranging between 580,000 and say 700,000, then the magnet which gives the largest product within this range will give the best results on a magneto. One perhaps should qualify this statement by saying that it only applies to magnets of a particular make, because recent experiments have shown that magnets furnished by different suppliers, even though the remanence and coercive force figures may be identical, can give different results when fitted to a magneto in consequence of the shape of the B-H curve under consideration being different in the two cases. That is, it would now appear that it is not sufficient to measure the remanence and coercive force only when considering the magnetic quality of a magneto magnet, but in addition one must take into account the trend of the B-H curve connecting these two points on the hysteresis loop. Taking an extreme case it is obvious that a magnet which gives a curve corresponding to (1) in Fig. 46 would give inferior results when fitted to a magneto as compared with another magnet having the curve (2) despite the fact that the remanence and coercive force figures are the same in each case. In the first case the active armature core flux would be proportional to the ordinate $OB_1$ and in the second case to the ordinate $OB_2$.

It is for this reason that I have recently introduced an additional test on magnets with a view to obtaining some indication of the trend of the B-H curve, and this test simply consists in measuring the active flux in an air gap, corresponding to the application of some definite demagnetising force. That is, a direct measurement is made of the ordinate $OB_1$ or $OB_2$ which corresponds substantially to the flux produced in the armature core of the magneto under normal working conditions. The statement in my paper to the effect that the armature core flux is dependent on the product of remanence and coercive force should therefore be qualified by stating that this only applies to magnets of one particular make which, generally speaking, give a B-H curve of some definite formation. In such cases this product can be taken as a criterion of the usefulness of a magnet for magneto work, and Dr. Bairsto may be interested to know that in a recent issue of the "Electrotechnische-Zeitschrift" there is an article on chrome steel magnets in which this product is taken as a basis of comparison between magnets made of chrome steel and tungsten steel respectively.

I do not agree with Dr. Bairsto's remarks that "glazing" means that the carbon brush is too hard, because all our experience goes to show that a brush

with a perfectly glazed contact face will not give rise to "tracking" when used with a suitable distributor material, and there will be no appreciable deposit of carbon dust or metal dust inside the distributor. If trouble is experienced with brass dust I would infer that the segment material is too soft, and this trouble could be overcome by using a harder metal, such as Admiralty gun metal, for example, which it is our standard practice to employ. Such a change would, I feel sure, be preferable to substituting another carbon brush made of softer material.

Mr. Brooks' remarks concerning the development of the magnet industry in this country, are of particular interest, and I am very pleased to know that prior to the outbreak of war such a large number of magnets for telephone generators were produced in this country from Sheffield steel. I am certainly surprised to learn that these magnets produced so long ago as 1905 were quite comparable to magnets now being manufactured in Sheffield for magneto work, and can only

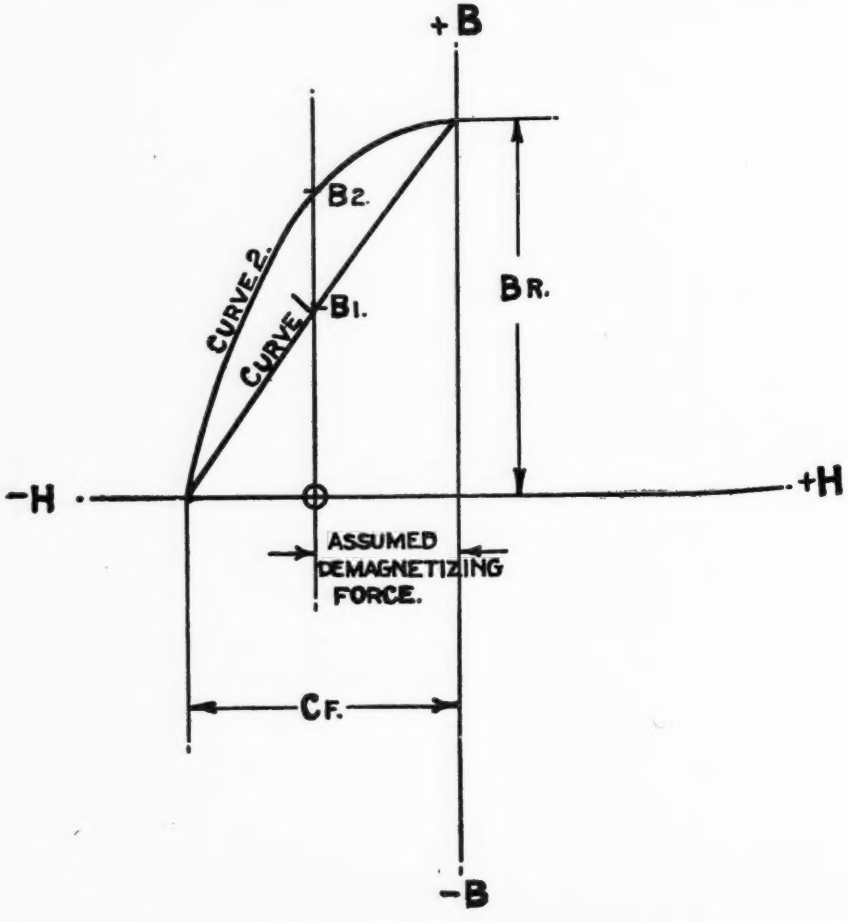



FIG. 46.

say that it is a great pity the information accumulated by Mr. Brook and his firm could not have been given wider publicity, for this should have eliminated, during the early stages of the war, the necessity of the Sheffield steel manufacturers carrying out so much experimental work in their endeavour to produce a really satisfactory magneto magnet for the requirements of the national service. This is obviously a clear case where co-operation between all those in a position to render assistance would have been of great national value.

I am in agreement with Mr. Brooks generally in his remarks concerning remanence and coercive force, and as he will see from my reply to Dr. Bairsto, given above, I place great importance upon the shape of the B-H curve and do not consider it is a difficult matter to quickly check the shape on a commercial instrument. Mr. Brooks thinks that a minimum coercive force of 55 is too low. He may be right for certain grades of steel, but all I can say is that the particular grade of steel that I have had most experience of gives entirely satisfactory results for magneto work, provided that the coercive force is not less than the figure stated. As Mr. Brooks states, the testing of magneto magnets has been a source of worry to Sheffield steel manufacturers—and incidentally to magneto manufacturers—and there is still room for hearty co-operation between all those concerned with a view to thoroughly establishing some simple and satisfactory workshop method of testing upon which all those interested can rely.

I do not think that Mr. Brooks' explanation of the drop in the energy output of the latest Bosch Zeppelin type ZH6 magneto, with increase of speed—as referred to in my paper—is the correct one, because the magnets on this particular machine gave a coercive force of 65 and a remanence of 10,780, showing that they were of excellent quality. The drop in the energy curve is entirely due to the eddy current damping at high speeds, and is accentuated in this particular construction by the soft iron segments in magnetic contact with the ordinary cast iron poles. As regards the inductor type of magneto, it is true, as stated by Mr. Brooks, that the energy output is low as compared with ordinary rotating armature type magnetos, but on the other hand the construction adopted gives a very much more rapid rise of secondary voltage at break, which in view of Mr. Morgan's and Dr. Bairsto's remarks—with which I am in entire agreement—is the factor of vital importance. The low energy output, therefore, is more than compensated by the improvement effected in respect of the shape of the secondary voltage wave, and the experience gained with this type of magneto on aeroplane engines confirms this view.

The loss in energy output resulting from the use of metal fixing strips, as referred to in my paper, applies to the case where the magneto is bolted to an aluminium base plate. If steel packing pieces be fitted underneath the magneto the loss in energy would be very greatly increased as a result of the additional leakage.

Lieut. Irving's remarks concerning the use of a spark gap type of distributor and a phosphor bronze bearing for the contact breaker lever are of interest. I am glad to know that he is in favour of the use of a phosphor bronze bush, and would add that all the experiments we have so far made show that a lubricated bush of this kind is very much to be desired to the ordinary fibre bush. With our design we do not work with as large a clearance as .004″, our limits for the difference in the two diameters being, maximum 0.0016″, minimum 0.0006″.

The principle underlying the construction of the Coates' inductor type of magneto could not, as I understand it, be used in designing high tension magnetos, because the voltage wave, if plotted for one revolution, is not symmetrical. That is, the flux does not actually reverse in the armature core, the flux curve being made up of a series of half waves. Although the machine would be satisfactory when used for supplying low tension current to an induction coil,

this inherent defect would prevent its being employed for high tension working in the ordinary manner.

Lieut. Irving's remarks concerning the necessity to re-magnetise the B.T.H. magneto after a 20-hour run are hardly relevant, because I distinctly stated in dealing with this matter that at the end of this preliminary run the machine is entirely dismantled, so naturally we have to re-magnetise the magnets before carrying out the final short run and subsequent low speed sparking tests. As a matter of fact, once the magnets have been magnetised there is certainly no need to re-magnetise them again during the working life of the magneto unless, of course, the machine is dismantled.

On the proposition of Sir CAPEL HOLDEN, Mr. Young was thanked for his lecture, and with a vote of thanks to the chair the proceedings terminated.

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**The Aviation Pocket Book.** By R. Borlase Mathews. (London: Crosby, Lockwood and Son. Sm. oct. pp. 300. 1917. 4s. 6d.)

Is a new and much improved edition of the Pocket Book published by Mr. Mathews in 1913. Our chief fault with it is in its name, but this is a venial offence, for it is unlikely to be confused with Moedebeck. The information given, however, is presented in a handy form and is convenient in character, and the book is well arranged. Apart from the usual technical information, there is a convenient bibliography, a directory of organisations, and a glossary of aeronautical terms. Altogether a useful book of reference.

**The Flying Book.** By W. L. Wade. (London: Longmans, Green and Co. Oct. pp. 292. 1917. 3s. 6d.)

This is a most useful compilation, profusely illustrated, and while by no means ambitious in form, manages to give a surprisingly large amount of information, tersely and well. Perhaps the main feature of the book, for it takes up nearly 100 pages of the volume, is a catalogue of "modern aeroplanes," British, French, German, Russian and American. A view and brief analysis is given of each machine. Other sections are the "Who's Who in Aviation," trade and other directories, and much miscellaneous information. The book may very fairly be classed as A1 and indispensable.